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MATHEMATICS AND THE WORLD.[1]

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MY object is to try to elucidate the nature of mathematical propositions, and to explain their relation to the everyday world of counting and measurement—of clocks, and yards of material, and income-tax forms. I should like to be able to summarise my views in a few short phrases, and then go on to defend them. Unfortunately I cannot do this, for, as I shall try to demonstrate, I do not think any short statement will do to express the truth of the matter with any precision. So I shall proceed by approximations—I shall discuss several different views in the hope that in showing what is right and what is wrong with them, clarification will come about.

The opinions of philosophers about the nature of mathematical propositions can be divided, as can their opinions about so many things, into two main classes. There are those who try to analyse mathematical propositions away—who say that they are *really* something else (like those writers on ethics who say that goodness is really only pleasure, or those metaphysicians who say that chairs and tables are really groups of sensations, or colonies of souls). I shall call such "analysing-away" theories "radical" theories. On the other hand there are those who insist that mathematical propositions are *sui generis,* that they cannot be analysed into anything else, that they give information about an aspect of reality totally different from any other (compare those philosophers who maintain, e.g., that goodness is a simple

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unanalysable quality, or those realists who maintain that a chair is a chair, an external material substance, known, perhaps, by means of sensations, but not to be confused with those sensations). For convenience, I shall call these types of theory which oppose any analysing-away, "conservative". I should maintain that in general what I call "conservative" opinions in philosophy are perfectly correct, but rather unsatisfactory and unilluminating, whereas opinions of the "radical" type are untrue, but interesting and illuminating.

I shall start by considering the "radical" theories about the nature of mathematics. Those I know of fall into two main types. (1) Some people maintain that a proposition of mathematics is *really* a particularly well-founded empirical generalisation of a certain type, or that it is logically on the same footing as a very well-established scientific law. Mill's theory was of this type, and many scientists I have talked to have tended to have similar opinions. Let us call these "empirical" theories about mathematics. (2) Then, on the other hand, there is a great variety of theories usually called "conventionalist", which analyse away mathematical propositions into propositions about the use of symbols. Examples: "By a mathematical proposition the speaker or writer merely expresses his intention of manipulating symbols in a certain way, and recommends or commands that others should do likewise." "A mathematical proposition is really an empirical proposition describing how educated people commonly use certain symbols." "A mathematical proposition is really a rule for the manipulation of symbols." (Ayer, for example, and C. I. Lewis have expressed opinions of this general type.)

First for the "empirical" theories. According to these a mathematical proposition just expresses a particularly well-founded empirical generalisation or law about the properties and behaviour of objects, obtained by examining a large number of instances and seeing that they conform without exception to a single general pattern. The proposition "7 + 5 = 12", for instance, just expresses (on one version of

this theory) the fact of experience that if we count up seven objects of any sort, and then five more objects, and then count up the whole lot, we always get the number twelve. Or again, it might be maintained that the geometrical proposition "equilateral triangles are equiangular" just expresses the fact that wherever, by measurement, we find the sides of a triangle to be equal, we will find, on measuring the angles with the same degree of accuracy, that the angles are equal too. It is contended that such propositions are essentially like, for example, Boyle's Law of gases, only much better founded.

But "7 + 5 = 12" does not mean the same as the proposition about what you get on counting groups. For it is true that 7 + 5 does equal 12, but it is not true that on counting seven objects and then five others, and then counting the whole, you will always get twelve. People sometimes miscount, and sometimes the objects counted melt away (if they are wax) or coalesce (if they are globules of mercury). Similarly the geometrical proposition that equilateral triangles are equiangular does not mean the same as the proposition that any triangle which is equilateral by measurement will be found to be equiangular when measured. The former is true; the latter false. We sometimes make mistakes with our rulers and protractors.

To this it might be objected that this shows that the empirical proposition offered as a translation of the mathematical one is not a correct translation, but that it has not been demonstrated that it is impossible to find an empirical proposition about counting and measurement, which is a correct translation. Let us try some alternatives, then. It might be suggested that "7 + 5 = 12" means "If you count *carefully and with attention,* you will get such and such a result." But, even with the greatest care in counting, mistakes sometimes happen, at any rate with large numbers. Shall we then say: "7 + 5 = 12" means "If you count *correctly* you will get such and such results"? But, in the first place, even if you count objects correctly, you do not always get a group of seven objects and a group of five adding up to twelve. It

sometimes happens that a person correctly counts seven objects, then correctly counts five, and then correctly counts the total and gets eleven. Sometimes one of the objects does disappear in the course of counting, or coalesces with another. And even if this were not so, the suggested translation would not give you a simple empirical proposition about what happened when people counted, as a translation of $7 + 5 = 12$, but would give you a mere tautology. For what is the criterion of correctness in counting? Surely that when you add seven and five you should get twelve. "Correctness" has no meaning, in this context, independent of the mathematical proposition. So our suggested analysis of the meaning of "$7 + 5 = 12$" runs, when suitably expanded: "$7 + 5 = 12$" means "If you count objects *correctly* (i.e. in such a way as to get 12 on adding 7 and 5) you will, on adding 7 to 5, get 12."

No doubt there *are* important connexions between mathematical propositions, and propositions about what results people will usually get on counting and measuring. But it will not do to say that a mathematical proposition means the same as, or is equivalent to, any such empirical proposition, for this reason: A mathematical proposition is "incorrigible", whereas an empirical proposition is "corrigible".

The difference between "corrigible" and "incorrigible" propositions can best be explained by examples. Most everyday assertions that we make, such as that "Mr. Smith has gone away for the day", are corrigible. By this I mean simply that, whenever we make such an assertion, however strong our grounds for making it, we should always freely withdraw it and admit it to have been false, *if* certain things were to happen. Thus my assertion, that Smith is away for the day, is corrigible, because (although I may have the excellent grounds for making it that when I met him in the street this morning he said he was on his way to the railway-station) if, for example, I were to go to his room now and find him sitting there, I should withdraw my assertion that he was away and admit it to have been false. I should take

certain events as proving, if they happened, that my assertion was untrue.

A mathematical proposition such as $7 + 5 = 12$, on the other hand, is incorrigible, because no future happenings whatsoever would ever prove the proposition false, or cause anyone to withdraw it. You can imagine any sort of fantastic chain of events you like, but nothing you can think of would ever, if it happened, disprove $7 + 5 = 12$. Thus, if I counted out 7 matches, and then 5 more, and then on counting the whole lot, got 11, this would not have the slightest tendency to make anyone withdraw the proposition that $7 + 5 = 12$ and say it was untrue. And even if this constantly happened, both to me and to everyone else, and not only with matches, but with books, umbrellas and every sort of object—surely even this would not make us withdraw the proposition. Surely in such a case we should not say: "the proposition $7 + 5 = 12$ has been empirically disproved; it has been found that $7 + 5$ really equals 11." There are plenty of alternative explanations to choose from. We might try a psychological hypothesis, such as this: we might say that it had been discovered by experiment that everyone had a curious psychological kink, which led him, whenever he performed counting operations of a certain sort, always to miss out one of the objects in his final count (like the subject in some experiments on hypnosis who, under suggestion, fails to see any 't's' on a printed page). Or we might prefer a physical hypothesis and say: a curious physical law of the universe has been experimentally established, namely, that whenever 5 objects are added to 7 objects, this process of addition causes one of them to disappear, or to coalesce with another object. The one thing we should *never* say, whatever happened, would be that the proposition that $7 + 5 = 12$ had been experimentally disproved. If curious things happened, we should alter our physics, but not our mathematics.

This rather sweeping assertion that mathematical propositions are completely incorrigible is, I think, an oversimplification, and needs qualifying. I shall mention the

qualifications later, rather than now, for simplicity of exposition. So if you will accept it for the moment as very nearly true, I should like to draw your attention to certain of its consequences. A *corrigible* proposition gives you some information about the world—a completely *incorrigible* proposition tells you nothing. A corrigible proposition is one that you would withdraw and admit to be false if certain things happened in the world. It therefore gives you the information that *those* things (i.e. those things which would make you withdraw your proposition *if* they happened) will *not* happen. An incorrigible proposition is one which you would never admit to be false *whatever* happens: it therefore does not tell you *what* happens. The truth, for example, of the corrigible proposition that Smith is away for the day, is compatible with certain things happening (e.g. your going to his room and finding it empty) and is not compatible with certain other happenings (e.g. your going to his room and finding him there). It therefore tells you what sort of thing will happen (you will find his room empty) and what sort of thing will not happen (you will not find him in). The truth of an incorrigible proposition, on the other hand, is compatible with any and every conceivable state of affairs. (For example: whatever is your experience on counting, it is still true that $7+5=12$). It therefore does not tell you which events will take place and which will not. That is: the proposition "$7+5=12$" tells you nothing about the world.

If such a proposition tells you nothing about the world, what, then, is the point of it—what does it do? I think that in a sense it is true to say that it prescribes what you are to *say*—it tells you *how to describe* certain happenings. Thus the proposition "$7+5=12$" does not tell you that on counting $7+5$ you will not get 11. (This, as we have seen, is false, for you sometimes do get 11.) But it does *lay it down*, so to speak, that *if* on counting $7+5$ you do get 11, you are to describe what has happened in some such way as this: *Either* "I have made a mistake in my counting" *or* "Someone has played a practical joke and abstracted one of the objects when

I was not looking" *or* "Two of the objects have coalesced" *or* "One of the objects has disappeared", etc.

This, I think, is the truth that is in the various "conventionalist" theories of mathematics. Only, unfortunately, the formulæ expressing such theories are usually misleading and incorrect. For example, to say that: "a mathematical proposition merely expresses the speaker's or writer's determination to use symbols in a certain way", is obviously untrue. For if it were true, and if I decided to use the symbol + in such a way that $5 + 7 = 35$, I would then be speaking truly if I said "$5 + 7 = 35$". But this proposition is not true. The truth of any mathematical proposition does not depend on my decision or determination. It is independent of my will. This formula neglects the "public" or "over-individual" character of mathematics.

Or, consider the formula: "A mathematical proposition is really an empirical statement describing the way people commonly use certain symbols." This, I think, is nearer. But it is open to the following obvious objection: If "$7 + 5 = 12$" were really an assertion about the common usage of symbols, then it would follow that $7 + 5$ would not equal 12 if people had a different symbolic convention. But even if people did use symbols in a way quite different from the present one, the fact which we now express by "$7 + 5 = 12$" would still be true. No change in our language-habits would ever make this false.

This objection is, I think, sufficient to show that the suggested formula is untrue, as it stands. But we should be blind to its merits if we did not see *why* it is that no change in our language-habits would make the proposition $7 + 5 = 12$ untrue. The reason is this: As we use symbols at present, this proposition is incorrigible—one which we maintain to be true whatever happens in the world, and never admit to be false under any circumstances. Imagine a world where the symbolic conventions are totally different—say on Mars. How shall we *translate* our incorrigible proposition into the Martian symbols? If our translation is to be correct—if the

proposition in the Martian language is to mean the same as our "7 + 5 = 12", it *too* must be incorrigible—otherwise we should not call it a correct translation. Thus a correct Martian translation of our "7 + 5 = 12" must be a proposition which the Martians maintain to be true whatever happens. Thus 7 + 5 = 12, and any correct translation into any other symbolic convention will be incorrigible, i.e. true whatever happens. So its truth does, in a sense, depend on the empirical fact that people use symbols in certain ways. But it is an inaccurate way of stating this fact to say that it describes how people use symbols.

A better formulation is "a mathematical proposition really expresses a rule for the manipulation of symbols". But this, too, is unsatisfactory, and for the following reason: To say that it is a "rule for the manipulation of symbols" suggests that it is entirely arbitrary. A symbolic rule is something which we can decide to use or not, just as we wish. (We could easily use "hice" as the plural of "house", and get on as well as we do now.) But, it seems, we cannot just change our mathematical propositions at will, without getting into difficulties. An engineer, building a bridge, has to use the standard multiplication tables and no others, or else the bridge will collapse. Thus which mathematical system we use does not seem to be entirely arbitrary—one system works in practice, and another does not. Which system we are to use seems to depend in some way not on our decision, but on the nature of the world. To say that "7 + 5 = 12" really expresses a rule for the use of symbols, suggests that this proposition is just like " 'house' forms its plural by adding 's' ". But there *is* a difference between the two, and so the formula is misleading.

I want to conclude this paper by considering in some detail the objection that you cannot build bridges with any mathematics, and that therefore mathematics does depend on the nature of reality. Before doing so, however, I should like to mention the type of theory I called "conservative". We saw that the (radical) theory, that mathematical propositions

are "really" empirical propositions about the results of counting, is untrue. But there is a close connexion between the two sorts of proposition, and therefore the "empirical" theory, although untrue, has a point. It emphasises the connexion between mathematical propositions and our everyday practice of counting and calculation; thus it serves as a useful corrective to that type of theory which would make mathematics too abstract and pure—a matter of pure intellect and Platonic "Forms", far from the mundane counting of change. Similarly the various "conventionalist" theories are also, strictly speaking, untrue, but they too have their point. Mathematical propositions in certain respects are *like* rules for the use of symbols, *like* empirical propositions about how symbols are used, *like* statements of intention to use symbols in certain ways. But conventionalist formulæ are untrue because mathematical propositions are not *identical* with any of these. They are what they are; they function in the way they do, and not exactly like any other sort of proposition.

And this it is which makes that sort of theories I have called "conservative" perfectly correct. Mathematical propositions are *sui generis*. But merely to say: "They are what they are" is not very helpful. Nor is it any better if this is dressed up in learned language: e.g., "Mathematical propositions state very general facts about the structure of reality; about the necessary and synthetic relations between the universals number, shape, size, and so on." If you are inclined to think that such answers as this, to the question "what are mathematical propositions about?", are informative and illuminating, ask yourself: "How does my hearer come to understand the meaning of such phrases as 'structure of reality', 'necessary relations between universals', and so on? How were these phrases explained to him in the first place?" Surely he was told what was meant by "necessary relation between universals", by being told, for example, that colour, shape, size, number, etc., are universals, and that an example of a necessary relation between universals would be "every-

thing that has shape has size", "2 + 2 = 4", "two angles of an isosceles triangle are equal", and so on. These phrases, such as "necessary relation between universals", are *introduced* into his language *via* or *by means of* such phrases as "2 + 2 = 4"; they are introduced *via* mathematical propositions, among others. To use an expression of John Wisdom's,* they are "made to measure". So to tell someone that mathematical propositions are "so-and-so" does not help, if, in explaining what is meant by "so-and-so", you have to introduce mathematical propositions, among others, as illustrative examples. Compare giving a "conservative" answer to the question "What are mathematical propositions?" with the following example: A child learns the meaning of the words "see", "can't see", "blindfolded" etc., before he learns the meaning of the word "blind". The latter word is then introduced into his vocabulary by the explanation: "A blind man is one who can't see in broad daylight even when not blindfolded." If the child then asks of a blind man "why can't he see in broad daylight even when not blindfolded?", it is not much use answering "because he is blind". Like the "conservative" answer in philosophy, it may serve to stop any further questions, but it usually leaves a feeling of dissatisfaction.

Then what sort of answer *can* be given to one who is puzzled about the nature of mathematics? Mathematical propositions are what they are, so any radical answer equating them with something else, such as symbolic rules, or statements of the results of counting and measurement, or of common symbolic usage, will be untrue. Such answers will be untrue, because the two sides of the equation will have different meanings. Similarly conservative answers will be unhelpful, because the two sides of the equation will have the same meaning. The definiens will be useless, because it will contain terms which are introduced into the language *via* the definiendum, and can only be explained in terms of it. It is

* My debt to the lectures of Wisdom and Wittgenstein, in writing this paper, is very great.

"made to measure". No simple formula will do. The only way of removing the puzzle is to describe the use and function of mathematical propositions in detail and with examples. I shall now try to do this, to some extent, in considering the natural objection to the strictly untrue but illuminating theory: "mathematical propositions express rules for the manipulation of symbols." The objection is that symbolic rules are essentially arbitrary, whereas mathematics does, to some extent at least, depend not on our choice of symbolic conventions, but on the nature of reality, because only our present system gives useful results when applied to the practical tasks of the world. Against this, I shall maintain that we could use *any* mathematical rules we liked, and still get on perfectly well in the business of life.

Example 1.—6×4, according to our current multiplication table, equals 24. You might argue: this cannot be merely a conventional rule for our use of symbols, for if it were we could use any other rule we liked, e.g. $6 \times 4 = 12$, and still get satisfactory results. But if you tried this alternative rule, you would, in fact, find your practical affairs going all wrong. A builder, for example, having measured a room to be paved with tiles, each one yard square, and having found the length of the sides to be 6 yards and 4 yards, could not use the alternative table. He could not say to himself: "The room is 6 by 4; now $6 \times 4 = 12$, so I shall have to get 12 tiles for this job." For, if he did, he would find he had not enough tiles to cover his floor.

But the builder could quite easily have used an arithmetic in which $6 \times 4 = 12$, and by measuring and counting could have paved his room perfectly well, with exactly the right number of tiles to cover the floor. How does he do it? Well, he:

(1) Measures the sides, and writes down '4' and '6'.

(2) Multiplies 4 by 6 according to a 'queer' multiplication table which gives $4 \times 6 = 12$.

(3) Counts out 12 tiles, lays them on the floor. And they fit perfectly.

The 'queer' multiplication table he uses gives $2 \times 2 = 4$, $2 \times 4 = 6$, $2 \times 8 = 10$, $4 \times 4 = 9$, $4 \times 6 = 12$ etc. The number found by multiplying a by b according to *his* table, is that which in *our* arithmetic we should get by the formula:

$$(a + 2)\ (b + 2)\ /\ 4$$

And he could pave any other size of floor, using the queer multiplication table described, and still always get the right number of tiles to cover it.

How is this possible? He measures the sides of the room with a yardstick as follows: He lays his yardstick along the longer side, with the 'O' mark of the yardstick in the corner, and the other end of the stick, marked '36 inches', some distance along the stick. As he does this, he counts "one". He then pivots the yardstick on the 36 inches mark, and swings it round through two right angles, till it is once more lying along the side of the room—this time with the "36 inches" mark nearer to the corner from which he started, and the "O" mark further along the side. As he does this, he counts "two". But now the direction of the stick has been reversed, and it is the convention for measuring that lengths should always be measured in the same direction. So he pivots the stick about its middle and swings it round so that the '36' mark is now where the 'O' mark was, and vice-versa. As he does this, he counts "three". He then swings the stick round through two right angles, pivoting on the '36' mark, counting "four". He then reverses its direction, as before, counting "five". He swings it over again, counting "six". It now lies with its end in the corner, so he writes down the length of the side as "six yards". (If we had measured it in our way, we should have written its length down as four yards.) He then measures the shorter side in the same way, and finds the length (using his measuring technique) to be four yards. (We should have made it three.) He then multiplies 4 by 6, according to his table, making it 12, counts out 12 tiles, and lays them down. So long as he uses the technique described for measuring lengths, he will always get the right number of tiles for any room with his 'queer' multiplication table.

This example shows you that we use the method we do for multiplying lengths to get areas, because we use a certain method of measuring lengths. Our technique of calculating areas is relative to our technique of measuring lengths.

Here you might say: admitting that this is true, it is still the case that mathematics is not arbitrary, for you could not use the method of measuring we do, *and* a different multiplication table, and *still* get the right number of tiles for our room. Could we not? Let us see.

Example 2.—Suppose our 'queer' multiplication table gave $3 \times 4 = 24$. The builder measures the sides of a room exactly as we do, and finds that they are 3 yards and 4 yards, respectively. He then "multiplies" 3 by 4, and gets 24. He counts out 24 tiles, places them on the floor, and they fit perfectly, with none over. How does he do it?

He measures the sides as we do, and writes down '3' and '4'. He "multiplies" and gets 24. He then counts out 24 tiles as follows: He picks up a tile from his store, and counts "one". He puts the tile on to his truck and counts "two". He picks up another tile and counts "three". He puts it on his truck and counts "four". He goes on this way until he reaches a count of "twenty-four". He then takes his "twenty-four" tiles and paves the room, and they fit perfectly.

This example shows that our technique of calculating areas is relative both to a certain technique of measurement, *and* to a certain technique of counting.

At this stage you might make a further objection. You might say: Mathematics *does* tell you something about the world, and is not an arbitrary rule of symbolic usage. It tells you that *if* you both count and measure lengths in the way we do, you will not get the right number of tiles for a room unless you multiply the lengths according to our present table. It is not arbitrary, because if, for example, you measure the sides of a room as we do, and find them to be 4 and 3, and if you count tiles as we do, you would get the wrong number of tiles to pave your room if you used some other multiplication table—say one in which $3 \times 4 = 24$. I

maintain, on the contrary, that we could quite well use such a 'queer' table, and count and measure as at present, and still get the right number of tiles. To help us to see what is involved here, let us consider a rather analogous case.

Example 3.—Imagine that the following extraordinary thing happened. You measure a room normally, and find the sides to be 6 and 4. You multiply normally and get 24. You then count out 24 tiles in the normal way. (Each tile is 1×1.) But when you come to try and lay the tiles in the room, you find that you can only get 12 such tiles on to the floor of the room, and there are 12 tiles over. What should we say if this happened?

The first thing we should say would be: "You must have made a mistake in your measuring" or "you must have made a slip in multiplying" or "you must have counted your tiles wrongly, somehow". And we should immediately check again the measurements, calculations, and counting. But suppose that, after the most careful checking and re-checking, by large numbers of highly qualified persons, *no* mistake at all of this sort can be found anywhere. Suppose, moreover, that this happened to everyone constantly, with all sorts of rooms and tiles. What should we say then? There are still a number of ways in which we might explain this curious phenomenon. I shall mention two conceivable hypotheses:

(1) Measuring rods do not, as we supposed, stay a constant length wherever they are put. They stay the same size when put against things the same length as themselves, and also when put against things larger than themselves running from north to south. But when put against things larger than themselves running east-west, they always contract to half their previous length (and this contraction happens so smoothly that we never notice it). Thus the room is in fact 6 by 2 yards, i.e. 12 square yards, and twelve tiles are needed. When the measuring rod is put along the north-south wall of six yards' length, it stays a yard long, and so we get a measurement of 6. When, however, it is put along the shorter east-west wall it contracts to half a yard in length, and can

be put four times along the two-yard wall. If you now say the dimensions are 6 and 4, and multiply to get 24, you are over-estimating the real area.

(2) An alternative hypothesis: When we measure the room our yardstick always stays a constant length, and thus the area of the room is really 24 square yards. But since we can only get 12 tiles in it, each tile being 1 yard square, it follows that the tiles must *expand*, on being put into the room, to double their area. It is just a curious *physical* law that objects put into a room double their area instantaneously. We do not see this expansion because it is instantaneous. And we can never measure it, by measuring the tiles, first out of the room and then inside, because our yardstick itself expands proportionately on being taken into the room.

This example (which might easily have been put in much more detail with *ad hoc* hypotheses to cover every discrepancy) shows that, however much the practical predictions of builders' requirements are upset when we use our present multiplication table, this need never cause us to alter our present rules for multiplication. Anomalies are accounted for by saying our knowledge of the relevant *physical* laws is defective, not by saying that the multiplication table is "untrue". If, when working things out in the usual way, we found that we had constantly 12 tiles too many, we should not say that we had been wrong in thinking that $6 \times 4 = 24$. We should rather say that we had been wrong in thinking that physical objects did not expand and contract in certain ways. If things go wrong, we always change our physics rather than our mathematics.

If we see, from example 3, what we should do if things went wrong when we used our present arithmetic, we can now answer the objection it was intended to throw light on. The objection was this:

"It is wrong to say that we could use any arithmetic we liked and still get on perfectly well in our practical affairs. Mathematics is not a collection of arbitrary symbolic rules, therefore, and does tell us something about, and does depend

on, the nature of reality. For if you *both* count and measure as we do, *and* use a 'queer' multiplication table, you won't get the right number of tiles to pave a room. Thus the proposition '$3 \times 4 = 12$' tells you that for a room 3 yards by 4, measured normally, you need neither more nor less than 12 tiles, counted normally. Its truth depends on this fact about the world."

But I deny this. I say we could have

(1) used our present technique of counting and measurement,

(2) multiplied according to the rule $3 \times 4 = 24$ (for example),

(3) and still have got exactly the right number of tiles to pave our room.

I therefore say that $3 \times 4 = 12$ depends on *no* fact about the world, other than some fact about the usage of symbols.

Example 4.—Imagine that we did use a 'queer' arithmetic, in which $3 \times 4 = 24$. If this was our universally accepted and standard arithmetic, we should treat the proposition $3 \times 4 = 24$ *exactly* as we now treat the proposition $3 \times 4 = 12$ of our present standard arithmetic. That is to say, if we did use this queer system, we should stick to the proposition $3 \times 4 = 24$ no matter *what* happened, and ascribe any failure of prediction of builders' requirements, and so on, *always* to a mistaken view of the physical laws that apply to the world, and *never* to the untruth of the formula $3 \times 4 = 24$. This latter proposition, if it *were* part of our mathematical system. would be *incorrigible,* exactly as $3 \times 4 = 12$ is to us now.

In example 3 we saw what would be done and said if things went wrong in using $3 \times 4 = 12$. Now *if* $3 \times 4 = 24$ were our rule, and incorrigible, and *if* in using it we found ourselves getting the wrong practical results, we should do and say exactly the same sort of thing as we did in example 3. Thus, assuming that our rule is $3 \times 4 = 24$, a builder measures his floor normally and writes down 3 and 4. He multiplies according to his table and gets 24. He counts out 24 tiles normally, and tries to put them in the room. He finds that he can only get 12 tiles in. What does he say? He *does not* say

"I have proved by experiment that 3×4 does not equal 24", for his proposition $3 \times 4 = 24$ is *incorrigible,* and no event in the world, however extraordinary, will ever lead him to deny it, or be counted as relevant to its truth or falsity. What he does say is something like this: "The area of the room is *really* 24 square yards. Since I can only get 12 yard square tiles into it, it follows that the tiles must expand to double their area on being put into the room." (As we have seen, he might use other hypotheses, e.g. about the behaviour of measuring rods. But this is probably the most convenient.)

Thus we could easily have counted and measured as at present, *and* used an arithmetic in which $3 \times 4 = 24$, *and* have got perfectly satisfactory results. Only, of course, to get satisfactory practical results, we should use a physics different in some respects from our present one. Thus a builder having found the area of a room to be 24 square yards would never attempt to put 24 tiles in it, for he would have learnt in his physics lessons at school that tiles put in a room double in area. He would therefore argue: "Since the tiles double in area, I must put half of 24 tiles, or 12 tiles, in the room." He would count out 12 tiles and pave the room perfectly with them.

But even here an obstinate objector might admit all this, and still maintain that mathematics was not an arbitrary convention; that it did depend on certain facts about the world. He might say: "$3 \times 4 = 12$ is true, and it is true because of this fact about the world, namely that *if* tiles and rulers do not expand and contract (except slightly with changes in temperature), and if we measure and count normally, we need exactly 12 tiles, no more and no less, to pave a room that is 3 by 4. And $3 \times 4 = 24$ is false, because of the 'brute fact' that *if* tiles etc. don't expand, and *if* you measure and count normally, 24 tiles are too many to pave a room that is 3 by 4."

The point that is, I think, missed by this objection could be brought out by asking: "How do we *find out* whether a tile or a yardstick has or has not expanded or contracted?" We normally have two ways of doing so. We can *watch* it

growing bigger or smaller. Or we can *measure* it before and after.

Now in the case described in example 4, where our queer arithmetic gives $3 \times 4 = 24$, and things double in area on being put in a room, how do we find out that the things do expand? Not by watching them grow—*ex hypothesi* we do not observe this. Nor by measuring them before and after. For, since we assume that a measuring rod *also* expands on being taken into the room, the dimensions of the tile as measured by a yardstick outside the room are the same as its dimensions as measured by the same (now expanded) yardstick inside the room. In this case we find out that the tiles expand by *measuring, counting and calculating in a certain way*—by finding that the tiles each measure 1×1, that the room measures 3×4, or 24 square yards, and that we can only get 12 tiles in it. This is our sole *criterion* for saying that the tiles expand. That the tiles expand *follows from* our queer arithmetic. Similarly, as we do things at present, our criterion for saying that tiles do not expand, is that when 12 tiles measuring 1×1 are put into a room 3×4, or 12 square yards, they fit exactly. From our present arithmetic, it follows that tiles do not expand.

In example 4, where we have a 'queer' arithmetic in which $3 \times 4 = 24$, and a 'queer' physics, it is a "law of nature" that tiles expand on being put into a room. But it is not a "law of nature" which describes what happens in the world. Rather is it a law "by convention", analogous to that law of our present physics which says that when a body falls on the floor with a certain downward force, the floor itself exerts an equal force in the opposite direction. It is just put into the system to balance our calculations, not to describe anything that happens.

This last objection might have been put in a slightly different form. It might have been said: "$3 \times 4 = 12$ does describe and depend on the nature of reality, because it entails a certain purely empirical proposition about what does and does not happen, namely the complex proposition: 'It is

not the case *both* that tiles do not expand *and* that we need less than 12 tiles to pave a floor measuring 3 by 4'." But I should maintain that this complex proposition (of the form 'not both p and q') is not empirical; that it does not describe anything that happens in the world, because it is incorrigible. Nothing whatsoever that we could imagine happening would ever lead us, if it happened, to deny this complex proposition. Therefore it does not tell us what happens in the world. The simple propositions which are elements in this complex one—the propositions 'tiles do not expand' and 'we need less than 12 tiles to pave a 3 by 4 floor'—are both of them corrigible, and both describe the world (one of them falsely). But the complex proposition that they are not both true is incorrigible, and therefore, for the reasons given earlier, does not describe or depend on the nature of the world. There is nothing out of the ordinary about this. The propositions "my curtains are now red over their whole surface", and "my curtains are now green all over" are both of them corrigible propositions, descriptive of the world. (One is true, the other false, as a matter of fact.) But the complex proposition "my curtains are not both red and green over their whole surface" is incorrigible, because nothing would ever make me deny it, and it is therefore not descriptive of the world.

I have talked, throughout the paper, as if mathematical propositions were completely incorrigible, in the sense that *whatever* queer things happened, we should *never* alter our mathematics, and always prefer to change our physics. This was a convenient over-simplification that must now be qualified. I maintain that we *need* never alter our mathematics. But it might happen that we found our physical laws getting very complicated indeed, and might discover that, by changing our mathematical system, we could effect a very great simplification in our physics. In such a case we might decide to use a different mathematical system. (So far as I can understand, this seems to be what has actually happened in certain branches of contemporary physics.) And mathematics does depend on and reflect the nature of the

world at least to this extent, that we would find certain systems enormously inconvenient and difficult to use, and certain others relatively simple and handy. Using one sort of arithmetic or geometry, for example, we might find that our physics could be reduced to a logically neat and simple system, which is intellectually satisfying, whereas using different arithmetics and geometries, we should find our physics full of very complicated *ad hoc* hypotheses. But what we find neat, simple, easy, and intellectually satisfying surely depends rather on our psychological make-up, than on the behaviour of measuring rods, solids and fluids, electrical charges—the "external world".

THINGS, PREDICATES AND RELATIONS.[1]

By G. F. Stout.

1. Things and Their Predicates.

The substance of what I have to say in this paper was originally contained in a correspondence which I had with Mr. G. F. McIntosh of Sydney. In the course of our discussion, there emerged one fundamental agreement between us. We both held that a thing as a subject of predicates is simply identical with the complex whole including all characters truly predicable of it—and I understood from Mr. McIntosh that he had derived this view from Professor John Anderson. Since it was common to both of us I had no occasion to support it by argument, but could assume it as a basis in discussing other questions. But as I am now addressing a wider circle, I shall begin by briefly indicating why I hold it. My reason is that I can find no tenable alternative. What is this orange which I assert to be yellow and round, juicy and edible? The natural view is that in asserting it to be yellow, round etc., I am giving a partial answer to this question, and that if I could assign all its predicable characters I should have a complete answer. But *ex hypothesi* this view is barred if we begin by assuming that the orange is something distinct, not only from its predicable characters, taken severally, but from all of them in their union with each other. On this assumption what possible answer can be given to the question, What is this orange as a subject of predicable characters? We can only say that it is "somewhat we know not what" related somehow we know not

[1] A paper read at the Annual Congress of the A.A.P.P. in Sydney University on Thursday, 15th August, 1940.

how to the characters that are said to characterise it. This statement is extremely obscure in the sense of being extremely indeterminate. But it does not necessarily follow that it is untrue or even unimportant. My point is that there is no such obscurity in the relevant facts. When I perceive or think of this orange, I am not perceiving or thinking of "somewhat I know not what"; and when I assert that this orange is yellow I am not asserting that it is related to yellowness somehow I know not how. Yet I can find no way of escape from this position unless I recognise that the orange is the whole complex including all its characters in their union with each other and that in asserting that the orange is yellow, I am asserting that "yellow" is a constituent of this character-complex.

What stands in the way of the general acceptance of this doctrine is the widely spread theory that predicable characters, as such, are universals. Hence it would follow that if a particular thing, e.g. this orange, is identical with the whole complex of its characters, it must be a complex of universals. This is rightly regarded as a *reductio ad absurdum*.[1] But if things are not complexes of universals, and if all their predicates are universal, it follows that the distinction between things and their predicable characters is identical with the distinction between particulars and universals. This is, I take it, the prevailing view at the present time. But it seems to me quite untenable. What is wrong with it is the theory that characters predicable of particular things are universal. I hold that they are as particular as the things of which they are predicates. When I see two oranges on the table before me, however similar they may be, I perceive them as two distinct things each occupying its own distinct place and separated by a distance. In just the same way I perceive the shape of each as distinct from that of the other, as occupying its own distinct place,

[1] Yet certain English Hegelians saw no absurdity in it. I remember having a discussion on this point with Professor David Ritchie, who asserted confidently that particular things are nothing and can be nothing but interpenetrations of universals.

and separated from other shapes by an intervening distance. There is no reason for asserting the oranges to be distinct particulars which is not also a reason for asserting their shapes to be distinct. Indeed I could not perceive the two oranges as distinct if I did not perceive the shape of each as distinct from that of the other. This is true even though the shape of each precisely resembles the shape of the other. For resemblance is a relation which must have at least two distinct terms.

If the characters of particular things are themselves particular, why are they so frequently assumed to be universals? The main reason is that we cannot name them or think of them without referring them to some general class or kind of character. In calling the shape of this orange "a shape" I am referring it to the general class *shapes*; in saying that it is spherical I am referring it to the more special class, spherical shapes. But it is the general kind of character which is universal. The instance of it which is found in this particular orange is not universal but particular.

It is a further question how general kinds of character are constituted. Do they involve a single common character which is numerically the same in all of them? Is there a single abstract character common to all shapes, to circles, triangles, parabolas, etc? Or if we deny this, is not the case in principle different for completely determinate kinds of character? If the shape of each of my oranges is precisely spherical, shall I say that there is a single abstract character common to both particular shapes? I raise the question only in order to point out that I need not here attempt to decide it. It does not affect the position that I am now maintaining. If there is such an abstract character it is predicable not directly of the orange, but only of the shape of the orange, which is neither abstract nor general but particular. It is a particular instance of the kind of character, called spherical shape. This remains true whatever account we may give of the way in which kinds of characters are constituted.

Assuming that to be a character predicable of a thing is to be a constituent of the character-complex which is identical with the thing, the next question to be considered is whether the relations of the thing to other things are, in this sense, predicates of it.

2. RELATIONAL CHARACTERS.

If in this sense there are no relational characters, then the thing-complex must be separately, independently and completely made up of non-relational characters. But on examination I find that this is not so. The shape and colour of a book may indeed belong to it independently of its being on this table; but only on condition that it is on something else—e.g. the floor—and that it has some other alternative spatial relation to the table. Indeed, as I shall insist hereafter, the non-relational character *shape* cannot exist at all without relation to surrounding space.

It would, I think, be generally admitted that there are relational characters belonging to the thing in the same way as the non-relational, were it not for a difficulty which may seem at first sight to be insuperable. It is an essential precondition of terms' being related to each other, that each of them must have a distinct existence and therefore a distinct nature of its own. Hence it may seem to follow that each of the terms must be self-complete antecedently to their relations, which are therefore externally superadded to them. I answer that we are here dealing with a character-*complex*. The question is whether this complex can contain relational as well as non-relational characters. I can find no reason why it should not. It cannot indeed include relational characters unless it also contains non-relational ones which distinguish it from the other things to which it is related. But if this condition is fulfilled, I can see no reason why various ways of being related to other things should not form part of the whole character-complex and be predicated of it in essentially the same way as its non-relational constituents.

In order to make this clear I must point out a very important ambiguity in the use of the term *relation,* an ambiguity which may lead to endless confusion if it is not cleared up. There is one meaning of the term *relation,* in which it is not predicable of either of the terms related, or of any of them if there are more than two. If a book is on a table, *on the table* is a relational character truly predicable of the book. But this is inseparable from another relational character predicable not of the book but of the table. How are these two relational characters connected with each other? We may be tempted to say that the difference between them is purely verbal, so that, whether we say the *book is on the table* or the *table is under the book,* we are merely expressing the same fact in different language. But this cannot be true; for *being on* is different from *being under*; killing from being killed; loving from being loved. Yet it is plain that a single indivisible fact is referred to whether we say that the book is on the table or that the table is under the book. This single fact may be indifferently described as the existence of the relation of *on* and *under,* or of *under* and *on.* This relation cannot be predicated of either of the related terms because, to adopt a metaphor due to Leibniz, it has one leg in each of them and is astride of both. It cannot be included in either of the related terms as a character in a character-complex. It is rather the relational or formal character of the complex whole which includes both of the related terms. We may therefore call it a total or enveloping relation. An enveloping relation cannot be within either of the terms it relates as a character predicable of that term. But each of the terms, inasmuch as they are related to each other, falls within the enveloping relation; each does so in its own way. Within the enveloping relation of *on and under* the book is to the table as *on* to *under*; the table is to the book as *under* to *on.* Now the fact that a term enters into an enveloping relation and enters it in its own way is a fact which characterises that term and nothing else. It belongs to it as one character in the character-complex which constitutes the being of the term.

Before leaving this topic, I feel that I ought to add something more, just in order to make my own position clearer without arguing or discussing difficulties. What I have named an enveloping relation is for me only the simplest case of what I call a form of unity. By a form of unity I mean the kind of complexity distinctive of a complex whole as distinguished from the constituents which it includes. The form of unity of a tune differs from that of a triangle. The form of unity of a heap of stones differs from that of a living organism or of an atom. In saying this I by no means intend to assert that a whole is any more than, or different from all its parts including the relations of its parts to each other. What I do assert is that this is true only on the assumption that the parts exist as parts of that whole and are related within that whole. What are potentially parts of a watch, mainspring, lever etc. as they lie on the table of a watchmaker do indeed form parts of a complex whole and are related to each other within that whole. But the whole is not a watch, and the mainspring, lever etc. as related within this whole are not constituents of a watch, and it is not true that the watch is identical with all of them in their relation to each other. It is true that a watch is all *its* parts. But they are not *its* parts until the watchmaker has put them together. Bradley asks—What relates a relation to its terms? One is tempted impatiently to dismiss the question as being merely puzzle-headed. But on one assumption it is by no means puzzle-headed, but raises a real difficulty. If we start by supposing that the terms are each of them independently complete in itself, so that relations are externally superadded to them, we are bound to ask how terms and their relations are connected with each other. And the question can only be answered by giving up the initial assumption which gave rise to it. What connects the terms with their relations is some complex whole which includes both the terms and their relatedness. The book is on the table and the table is under the book only in and through the complex situation which includes both table and book and their relatedness to each

other. On the other hand, what exists as a part of a complex whole must therefore be related, however indirectly, to other parts of that complex whole. Let us assume that there is one all-inclusive complex of which whatever else exists is part. Then the parts of this complex must, as such, be all in some way related to each other. In order to be more definite, let us assume that this all-inclusive complex is what Alexander calls Space-Time. On this view it belongs to the nature of everything which is not Space-Time as a whole to be a part of Space-Time, and therefore to be spatially and temporally related to every other part. Such relatedness must therefore be predicable of each thing in the same way as its non-relational characters.

3. CONSTITUTIVE RELATIONS.

So far I have considered relations in general and my main point has been that things have relational characters predicable of them in the same way as those which are non-relational, i.e. as constituents of the character-complex which *is* the thing. When in ordinary language we speak of relations, we nearly always mean relational characters, not enveloping relations, and I shall in what follows conform to ordinary usage, unless I give express warning.

There are at least some relations which I hold to be in a special sense *constitutive*. I call a relation constitutive if some non-relational character would not be what it is in the absence of a certain relational character. Whether there are or are not relations which are in this sense constitutive can only be decided by an appeal to experience, involving analysis of examples.

Take the proposition *A loves B*. "Loves" in this proposition is sometimes given, e.g. by Russell, as a typical example of a relation. On this view the relation of A to B is "loving B". But plainly "loving" is not merely a relation. It is an emotion and desire or the complex system of emotions and desires which Shand calls a "sentiment". Neither an emotion nor a sentiment is merely a relation. But apart from

relation to a beloved object, an emotion or a sentiment cannot have the distinctive quality of love. Conversely, if there is no loving nothing can be loved. The emotional and the relational factors form a single complex within which each is indispensable to the other. Shape supplies another example. Consider what a shape is, e.g. the shape of the letter S as I now write it. Shape is not merely a relation; for it essentially involves a qualitative character. But there is a kind of relation such that when it is present shape is present, and when it is absent shape is absent. This relation is the demarcation of a place from an adjoining place by a qualitative difference as the black letter S is bounded in a special way by the whole background of the white paper on which it is written. Since I am here only concerned to show in what sense I use the term constitutive relation, I shall not give further illustrations, which might be multiplied without end. I shall only add that the equation to a curve seems to me a quite undeniable example.[1]

In the above statement I have only stated my own view. I am not prepared to make myself responsible for what others may mean by a constitutive or, as they may more usually say, an *internal* relation. But I hope and believe that very few who hold that there are such relations would accept the definition of them frequently given by their opponents. If anyone says that "a relation together with one of its terms is logically sufficient to account for the other term", he is talking nonsense. The relation cannot account for the other term because it cannot itself exist without the other term. But who really holds this? Certainly not all those who have been accused of holding it. On one interpretation of Hegel, it is a Hegelian doctrine. But I very much doubt this interpretation. Of the English philosophers who have been influenced by Hegel, perhaps T. H. Green is really guilty of this absurdity. Once posit a self-distinguishing activity, and it is impossible to say what acrobatic feats it may not be capable

[1] Also the scientific definitions of such concepts as *mass*, *energy*, *weight*, etc.

of. But the doctrine is not, I think, to be found in Bradley or Bosanquet. They seem to mean by internal (constitutive) relations very much what I mean. But they hold that *all* relations are in this sense in some manner and degree internal. This is required by their philosophy, but not by mine.

4. THE CONCRETE INDIVIDUAL AND THE CONVERSION OF PROPOSITIONS.

I hold with Aristotle that a concrete individual or *thing* is not predicable of anything else. A concrete thing is a character-complex, and to be a character predicable of the thing is to be a character contained within this complex. But this complex itself is not included in any more inclusive character-complex as a constituent character predicable of it. Hence a concrete individual cannot be a predicate of anything else. If we accept Alexander's definition of a thing, as a separate portion of Space-Time, what I am saying is that no separate portion of Space-Time is predicable of any other because each of them is a character-complex complete in itself. This book cannot be a predicate of this table. Even when one concrete thing is part of another, it is not predicable of the whole which includes it. My two legs are not predicable of me. What is predicable of me is that I have two legs or am two-legged or a biped. It is true that my body may be exhaustively analysed into such constituents as legs, arms, nerves, mind, heart, lungs, etc. But this analysis is not into predicable characters, but into a quite different *set of parts*. Similarly a living organism is all its component organs taken together and it is also all its component tissues. But tissues and organs are different *sets of parts*.

If a thing cannot be a predicate, it follows that propositions in which a thing or things are the subject cannot be converted. Many philosophers, including Professor Anderson, agree with the traditional Logic in holding that they can. To make my position clear, I must define precisely the question at issue. There are two points that have to be constantly borne in mind. (1) To be a predicate of a subject

is to be a character asserted to be part of the whole character-complex which *is* that subject. (2) In conversion, strictly so called, the subject of the converse must be precisely what was the predicate of the convertend and nothing else. Now consider the proposition, *This rose is red.* Its converse, if it had one, would be, *Red (or redness) is this rose.* This may be taken to mean either that redness is identical with the whole character-complex which constitutes this rose, or that the redness is a character-complex which contains the rose-complex as part of itself. Both interpretations lead to nonsense. Accordingly the text books require us, before converting such a proposition, to perform an operation which they absurdly call "stating the proposition in logical form". The conventional rule is that we must substitute instead of *This rose is red,* the proposition, *This rose is a red thing.* Then the converse is said to be, *Some red thing is this rose.* But when I say *This rose is a red thing,* I am not asserting but only implying that redness is a predicate of this rose. What I assert is that this rose is identical with some one member of the class *red thing* without specifying which. (*A red thing* simply means some one member of the class *red things*). There is a systematic ambiguity in the use of the verb *to be* as a grammatical copula. When the grammatical copula "is" is followed by an adjective or adjectival phrase, it means that the character for which the adjective stands is included in the character-complex which is the subject, i.e. is a predicate of that subject. When "is" is followed by a substantive, it means "is identical with". But in the proposition *A is identical with B,* the predicate is not *B,* but *identical with B.* The conversion of this according to conventional rules should be *Something identical with B is A.* But this is only a pleonastic way of saying that *B is identical with A.*

5. RELATION TO EACH OTHER OF THE CHARACTERS OF A CHARACTER-COMPLEX.

The complex unity of a character-complex is of a unique kind, differing from any other kind of complexity. Hence the

several characters which it includes are related to each other as they are related to nothing else. The shape and colour of a leaf are related to each other and to other characters of the leaf in a peculiarly intimate way in which neither of them is related to the colour or shape or to the other characters of a different leaf. Without such relations internal to the complex, a character could not be a character at all. Shape and colour and texture would not be the shape, colour or texture of a leaf if they were not, each of them, related in such a peculiarly intimate way to each and to all of the other characters of the leaf. Such relations may be called constitutive in the sense that they constitute any predicable character as such. But no character can consist merely in its relatedness to other characters of the same complex. Each must have a distinctive existence of its own, differentiating it from the others. If it were not distinct from them, it could not be related to them. In this way every predicable character is complex, however simple it may be in other respects. It must include both relatedness to other characters and also what differentiates it from them. Both these factors are indispensable to its being a character at all. For this reason, neither by itself is predicable of a thing, though both are predicable of any character of the thing. To assert a character of a thing consists in asserting its relatedness to other characters of the same thing. Such relatedness cannot therefore be predicable of the thing itself.

It follows also that no character of a thing is predicable of any other character except in the special case that one is complex in such a way as to contain the other. With this reservation, what is predicable of any character is only its relatedness to the others. The colour of honey is not predicable of its sweetness; the wisdom of a man is not predicable of his stature.

It is convenient to fix on a general term to indicate the peculiarly intimate way in which characters of the same thing are related to each other. *Interpenetration* has been proposed. But this would strictly mean that any two characters overlap

in a part common to both, which is by no means always true. I suggest *concrescence* as the most appropriate term. Characters concresce in a concrete thing.

6. So-called Identical Propositions and Theory of Predication.

I have in the last place to consider an objection to my theory of predication which if it were valid would be fatal. It may be urged that if the predicate is contained in the subject all subject-predicate propositions must be what are called identical propositions. Now I agree with Professor Anderson that a so-called identical proposition is not a proposition at all, but an idle repetition. It is merely idle repetition to say that this paper is this paper, or that this white paper is white. There is the verbal form of an assertion, but nothing is asserted. To name as if it were an asserted predicate, a character which is at the same time being taken for granted as belonging to the subject, is to use a verbal formula, void of significance. Whether we say that *A is A* or that *AB is B,* we have only a grammatical sentence which does not express a proposition. But on my view of predication it may seem that all subject-predicate propositions must be of the form *AB is B,* where B stands for the asserted character and A for the other characters of a character-complex. Before dealing with this difficulty I should like to consider what possible alternative there is to my view. If B is not included in a character-complex A, what can be meant when we say that A is B? We do not assert that A is identical with B, e.g. that a rose is identical with its redness. We do indeed assert that the predicated character is related to other characters of the same thing. But the only relevant relations are those in virtue of which it is one constituent among others of the same character-complex. In asserting such relations we are asserting that it is included in this complex.

Are we therefore committed to the view that all true propositions are tautologous because they take the form *AB is B*? There is no way of meeting this difficulty except by

taking account of the subjective aspect of propositions. But when we do take account of the subjective aspect, the answer is easy and obvious. There is tautology only when what grammatically appears as if it were a predicate is already taken for granted without question as characterising the subject. The clearest case where there is no tautology is that in which the opposite has been previously taken for granted. Those who first discovered black swans in Australia had previously assumed that all swans are white. In asserting the proposition "some swans are black" they were asserting what was new to them and therefore not a tautology, though of course the objective fact was not new. But this is only a special case. There need be no tautology even when an individual asserts what he already knows or believes. There is tautology only when what appears grammatically as a predicate is at the same time taken for granted as characterising the subject. In asserting that *A is B*, I assert explicitly that B is included in the character complex A in union with whatever other characters may belong to this complex. But the others are not explicitly asserted in saying that *A is B*. So far as they are present to the mind at all they are taken for granted. They are taken for granted inasmuch as no question is raised whether they belong to A or not. I do not explicitly assert that *A is B* unless the possibility of A's not being B is thought of, however faintly and transiently. I may initially be ignorant whether A is B or not, or I may not feel sure that it is so. When I myself begin by assuming that A is B without any thought of an alternative, the thought of A's not being B may be suggested to me from without. For example, some one else may deny or doubt that A is B. Or he may behave as if he did not believe it; he may for instance try the handle of a door which I know to be locked, thus provoking me to assert "That door is locked". The ways in which the alternative possibility may occur to the mind are manifold. But all explicit assertion comes as the answer to a question, however the question may be raised. And just because it answers a question it is not a tautology.

It follows that a proposition does not consist merely in the existence or non-existence of an objective fact. It also essentially involves the relation of objective fact to a mind capable of believing, disbelieving, doubting, entertaining a question, or making a supposition. Such relation to a mind is constitutive of a proposition as such, though it does not of course determine what the proposition is. It does not determine whether a proposition is true or false. But apart from it there is neither truth nor falsity.

THE PROBLEM OF ANALYSIS IN PSYCHOLOGY.[1]

By Duncan Howie.

If we are to seek common elements in the diverse, we must first loosen or unbind the complex into the simple. Analysis is an essential of thought and systematic analysis must be the mark of the systematic thought of science. It is just in this that the exact sciences have been so effective: by rigorous analysis of their data they are able to describe events in exact and uniform terms. A science, then, cannot be developed till it is capable of identifying those elements which in combination in nature constitute the phenomena with which it deals. Without such a reduction to elements, the determination of uniformities is impossible. The science lacks a common language. The data become this to me and that to thee, since I offer analysis to one kind of element, you to another. As Spearman puts it, the irreducible minimum for science in general is "a proven account of what goes with what".[2]

The problem of analysis in psychology would, therefore, appear to be simple enough: get on with analysis till you can offer such elements. And this is all that it meant for Titchener:

> The psychologist seeks first of all to analyse mental experience into its simplest elements. He takes a particular consciousness and works over it again and again, phase by phase, and process by process, until his analysis can go no further.[3]

But set against this Koffka:

> The analytic method can, according to this way of thinking, only find out into what kinds of reactions other reactions can be transformed by the change of attitude called psychological analysis.[4]

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[1] A paper read at the Annual Congress of the A.A.P.P. in Sydney University on Friday, 16th August, 1940.

[2] Spearman, C.: "Psychology Down the Ages", II, p. 205.

[3] Titchener, E. B.: "Textbook of Psychology" (1910), p. 37.

[4] Koffka, K.: *British Journal of Psychology*. 1924-5, 15, p. 155.

I am not at the moment concerned with the differences of these two schools. The opposed viewpoints are quoted to indicate that the problem of analysis in psychology presents peculiar difficulties; it is not simply a question of technique.

The trouble certainly is not any failure on the part of psychologists to recognise the importance of analysis, or any lack of zeal and thoroughness in the prosecution of analysis. But the result is a bewildering variety of elements proposed. Are we to reduce all to sensation, image, feeling: or to stimulus-response bonds with neural correlates: or to reflexes and the principle of conditioning: or to instincts, fundamental innate properties: or to factors, the modern faculties? It would be wearisome to continue the list, but clearly even this partial indication of the diversities resulting from efforts at reduction to elements suggests that there is something seriously the matter with procedures of analysis in psychology.

One downright objection is that all methods of analysis are inapplicable seeing that they are trying to describe in terms of parts what is essentially a unity and cannot be so reduced. The objection comes from varied sources, from all those impressed, in one way or another, with the continuity, the essential belongingness-together of experience, whether as an emphasis on the psychological act as unique and not to be grasped by any study of contents, or as taking all the data of psychology to be the expression of an experiencing and active self, or in a more general recognition that organisation is a fundamental aspect of all that we call mental. With differing views of what the field of psychology is and of what the psychologist should be doing, all unite in scorning the necessarily artificial nature of elements resulting from analyses and the failure of any resynthesis of these elements to give experience as experienced and behaviour as behaved. They argue that, in the effort to explain through analysis to elements and the discovery of the laws of their interconnection, that which was to be explained has escaped in the process.

An extreme but, perhaps, a natural development of such a viewpoint is seen in the modern German cleavage between

psychology as a natural science and as a cultural science. Dilthey, for example, maintains that the method of natural science (reduction to elements) cannot deal with all that is summed up in the concept *value* which must be the major consideration of a cultural science. For this not explanation, but understanding becomes of first importance. In effect, the "understanding" approach is the artistic approach: an attitude of appreciation which precludes analysis of the natural science kind.

I cannot believe that an attitude of this sort has much to offer beyond its negative value as a protest against misleading atomism. Beyond this, however interesting and inspirational such contributions may be, merely to emphasise continually unity, integration, understanding, appreciation, and so on, denying either implicitly or explicitly the possibilities of systematic analysis, is but to offer rhapsodic repetition of a text with copious illustration, to affirm a psychological faith in a series of sermons. Granted essential unity, totality, organisation, what can we do about it? Even if we cannot reduce the whole to discrete parts, we may, in fact we must, describe it, for even to talk about it is in some degree to describe it. Now description of course involves analysis, if only in the selection from among the many ways in which the given whole may be viewed of those which appear most adequately to indicate its essential nature, or which serve best for its identification. Thus; a given leaf may be described as having a certain greenness and a certain shape. The explicit statement of these qualities will enable others to identify this leaf. In this we have analysis to certain elements for purposes of description, but there is no suggestion that the leaf is made up of greenness and shape. We could surely discuss the particular shade of greenness, the particular shape of a given leaf and possibly relate these to climatic or other conditions of growth; we could classify different leaves in terms of their greenness and shape; we might even be able to show in certain cases a constant relationship between shade of green and characteristic shape. All this, I believe, would be of consider-

ably more value than eloquent affirmations that the essence of a leaf is its leafiness. There is no need, I take it, to labour the analogy. The application to discussions on personality, self, unity of consciousness, etc., is plain enough. No matter how much we emphasise the essential unity of mental process, the problem of analysis remains. It is not a question whether to analyse or not to analyse, but of the kind and degree of analysis. Incidentally, the leaf analogy is helpful in considering one aspect of the dispute about factorial analysis. The objection that the unity of mental functioning cannot be reducible to uncorrelated factors becomes no longer formidable if we accept factors as descriptive not constitutive elements. By descriptive elements I mean such points of reference as have empirical definition and enable us to identify, to compare, classify and relate the data of experience and behaviour. My general argument is, that, for the most part, the confusion of schools in psychology is the result of premature attempts to push beyond a descriptive level to one of ultimate explanatory units, whose combination is taken to constitute the data of psychology.

The trouble is, psychology as a young science is like most youngsters self-conscious about its youth and, therefore, anxious to appear mature by imitating its elders. The success of the methods of detailed observation and analysis in the natural sciences, particularly physics, showed it a model to copy. Here there was selection of certain aspects of experience to the rigid exclusion of all else, together with the thorough-going analysis of the selected field. A psychologist, too, was entitled to select his field. On this model, selection was inevitably determined by the degree to which it appeared amenable to analysis. Then, with material which could be analysed all that remained to do was to prosecute the analysis with complete rigour till ultimate elements were reached.

In the history of the different ways of analysis there are certain clearly marked stages.

First, a preliminary theory shows a value in analysis at a general descriptive level.

The hint is caught up and the line of analysis developed enthusiastically, till elements become more ultimate and laws of connection more comprehensive, so that what was originally, perhaps, suggested as a principle of restricted scope becomes the fundamental principle for psychology. Here, it is claimed, is the final method, here are the ultimate elements, and the ultimate laws, and these are all that is required to explain even the most complex and abstruse of psychological problems.

There follows a period of uneasiness. Constant criticism is making itself felt. Even its upholders become conscious of the inadequacies in the scheme. There are attempts to meet difficulties by stretching concepts often to the limit of tenuity, or by adding another element or another law or so, not always with complete consistency.

Finally, the stretching process becomes too much for the framework, and generally by this time there is a more popular system offering.

These stages are nicely shown in the vicissitudes of associationism.

Locke we may take as the starting point:

> Even the most abstruse ideas, how remote soever they may seem from sense, or from the operations of our mind, are yet such as the understanding frames to itself by repeating and joining together ideas that it had, either from the objects of sense, or from its own operations about them.[1]

There is, of course, much in the passage that further development on associationist lines will decisively reject, i.e., the reference to the operations of mind, the acceptance of reflection as a source of ideas. However, the root of associationism is certainly there.

Locke's general descriptive treatment develops into the complete dogma of associationism with Hartley. The capacity for sense experience is all that is needed. The fundamental elements are sensory elements, paralleling neural vibrations; the laws of "repeating and joining" are summed up in one law, contiguity in experience. Thus, a descriptive statement is

[1] "An Essay Concerning Human Understanding", Book II, Chap. XII, 8.

developed into a fundamental principle of explanation. These principles are rigorously applied in Condillac's famous statue which represents the climax of the associationists' confidence in the adequacy of their explanatory scheme. His statue given sense has all else added unto it. From the mere succession of experiences all mental processes may be derived.

Subsequent associationists, with the exception of James Mill, hardly maintain this level. The highly specialised and selective analysis discovers its own weakness. In different ways there are efforts to overcome these, and in varying degree there is recognition that the observed facts of synthesis, organisation, activity are not very adequately explained in terms of a passive linkage of bits of experience. There are efforts to make the scheme more flexible while still clinging to the beautiful simplicity of aggregated elements.

The uneasiness is quite apparent in Wundt. Philosophically a Kantian, he fully recognises mental activity. Mind is actual not substantial. But his methods are the methods of natural science: the analysis to elements, the determination of the manner of connection of these elements, the determination of their laws of connection. The elements, however, whether sensation, image or feeling, are not to be considered as static bits of mind stuff, but as elemental processes. One cannot but agree with Boring that this conception is an ambiguous one. It would appear impossible to hold elements arrived at by introspective analysis as processes; the material before attentive observation must appear as a content, and there seems no escape from the analysis of contents to a mosaic of bits. To describe elements as processes is only a verbal juggling with the real difficulty of explaining the flow of experience in terms of elements. This is an example of calling upon "natural piety" to supplement the inadequacies of a rigid explanatory system dealing with observed data.

So, too, it has often been objected that Wundt has been quite unfairly made the target of the gestaltists, seeing that, whoever may have believed in a "bundle of hypotheses", one has only to quote his law of psychic resultants or creative

synthesis to show clearly that he did not. The law states that the combination of elements gives rise to resultants which have properties other than the properties of the separate elements. This is all very well but quite fails to answer the gestaltist argument that explanatory concepts based on discrete elements cannot account for the properties of organised wholes. Wundt's law is yet another statement of "natural piety", not a principle in the light of which his methodology is developed.

In passing, it is worth noticing that the point raised here has implications beyond the immediate question of Wundt and associationism. Quite a few psychologists warmly welcome theories such as emergent evolution, holism, organicism, as means of making the best of two worlds, the world of analysed elements, and the world of order and value. Often the theories are not referred to in terms of any particular 'ism but are implied in the very loosely used concept "integration". Now, it is one thing to make the principle of organisation basic and devise techniques adapted to its study, as I think Koffka, Köhler and Lewin are making a valiant effort to do. It is quite another thing to be committed to an elementaristic approach, and attempt to shuffle off the difficulties by an appeal to a more or less explicit philosophical theory. It amounts to this, that having insisted on smashing Humpty Dumpty to bits, and being unable to reassemble him, we solemnly affirm that while these fragments in themselves do not constitute Humpty Dumpty, their nature is such that they essentially tend to organise themselves into Humpty Dumpty or have "a nisus" to Humpty-Dumptiness.

To return to Wundt. Wundt is at the troubled stage in the development of the associationist line of analysis. On the one hand, he has given it new life by wedding it to experimental method; on the other, there is evidence of uneasiness, efforts to break down the rigid discreteness of the elements (e.g., complication, fusion, assimilation, etc.), and statements of principles whose consistency with this method is questionable.

In Wundt the set of associationism is strong but already there are signs of its breakdown.

The strength of this associationist set is interestingly shown in the imageless thought controversy. In the earlier stages the Würzburg school were arguing, not in the way of a general attack on associationism or elementarism, but for the right to include new elements, thought elements. Viewed in historical perspective, however, the movement represents a complete questioning of the adequacy of the whole associationist procedure to account for the flow and direction of thought.

The increasing strength of hostile forces reaches its climax in the mass break-away of American functionalism, but the old tradition is maintained even more intransigeantly in Titchener's structuralism. Persistent difficulties are solved by excluding them from consideration. Psychology is strictly limited to the study of the conscious states by a specialised process of introspective analysis. Whatever cannot be reduced to sensation, image, feeling, is not psychological. The old objection that we don't and can't experience isolated elements, that experience is just what it is and, as experienced, does not come as made up of bits of experience, is met by taking the elements to be conceptual not phenomenological. So far has associationism developed from Locke! From a simple descriptive account of experience as analysable to elements, themselves items of experience, we arrive at an explanatory system in terms of elements that have not been and cannot be experienced.

It is not within the scope of this paper to enter into a technical discussion on the questionable assumptions, for example the "constancy hypothesis", on which the procedure of existentialism is based. Nor can I argue here the value and limitations of conceptual hypothesis in psychology. To criticise the adequacy of the school even within its own restricted sphere would mean a tedious retreading of the old ground already thoroughly covered by James, Ward, Stout, and especially by the gestalt school with their overwhelming weight

of experimental evidence. With regard to the claim of existentialism to be the only true science of psychology, whatever else may be said, in its effort to be scientific at all costs it has sacrificed the consideration of practically all those problems which, historically, have constituted the main urge to psychologising.

To recapitulate. The story of associationism from Locke to Titchener is briefly this: that an analytic procedure, at any rate defensible at a descriptive level and, most would agree, of considerable value at that level, becomes so sophisticated as an effort towards ultimate explanation that it manifestly fails to deal completely with observed data. Its rigorous, selective analysis becomes a hindrance to a clearer view of the whole field.

I suggest that this is a general criticism of the schools; principles valuable enough at one level or for a restricted set of data become distortions when put forward as ultimately explanatory concepts. The confusion in psychology, is, I believe, due to attempts to push beyond description to effect a systematisation in terms of fundamental explanatory units, and such attempts, at any rate in so far as each claims to be the one royal road, must be deemed premature.

In support of this contention, I shall cite three further examples: the conditioned reflex principle, the instinct theory, and the Spearman-Thomson dispute about the general factor "g".

Behaviourism was the natural child of functionalism. Functionalism turned from the problem of structuralism which had landed in the sophistications of Titchener and sought a more workable approach to mind in terms of function, the ways in which the mind (or, if they disliked that term, the psycho-physical organism) operated as a process of adjustment to environmental conditions. This line is, I believe, fundamentally valuable and, one way or another, any attempt to study function, to deal with mind in use, unless it be barren exercises in classification of the old faculty kind, must relate mental processes to the modes of action by which an

individual maintains its integrity in its environment. Variations in mental process can only be given empirical significance in relation to variations in environmental conditions. In this sense, the formula stimulus-response is a convenient summary of the psychological problem and approach. But what does it become in the hands of the behaviourists? A reduction to reflexes as ultimate units and conditioning as the general principle of association. Here, again, as descriptive of certain observed phenomena, there can be no real objection to either term. The reflex is an observed fact, a clearly defined mode of relatively stereotyped response basic to the survival of the organism and therefore unlearned. Nor is there any question of conditioning as a descriptive statement; reflex responses may be elicited to all sorts of stimuli which in themselves bear no natural relationship to that type of stimulus to which the reflex normally occurs. When, however, these descriptive terms are offered as final and complete explanations of all mental functioning, we are back again at associationism, with reflexes for elements, and conditioning, the new name for contiguity.

Note, too, that much of the plausibility of behaviourism is due simply to a quite loose usage of the term stimulus. "How often has 'a mouse', 'a door', the 'experimenter' and so on been called 'the stimulus' in animal psychology", Köhler[1] remarks. The term is extended beyond that precision and definiteness which is essential for a true behaviourist position to cover almost any complex to which the organism responds. Köhler argues convincingly that so used the expression will go far to hide real problems of organisation underlying the response. In particular, behaviourist discussions on social psychology or problems of personality seem little more than analogy. Here, again, we have a simplification whose apparent comprehensiveness and precision are largely illusory.

The same holds for McDougall's instinct theory. As a descriptive fact it is true that there are certain conditions which an organism must satisfy in order that it may

[1] Köhler, W.: "Gestalt Psychology", p. 138.

survive or its species continue; for example, it must seek food, it must avoid danger, it must mate, it must care for its young. In so far as a species has survived it would appear to have developed means of meeting these requirements, and, because many of these requirements are universal for a species, it would follow that each member of the species has the possibility of developing satisfactory means of meeting these requirements, i.e., each is so constituted as to seek in its environment conditions requisite for its survival and development. Light is necessary to plants, they are made that way; therefore, they grow to the light. At the human level, if man is by nature a social animal, he needs society, and, therefore, he is so constituted as to seek it.

This, I take to be the truth in instinct theory. I believe, too, that there is considerable value in a classification of the main requirements of social life and thus of the main needs of human nature. It is undoubtedly useful in clinical cases to know in what general requirement an individual falls short, or what need in him is unsatisfied or excessive. In this way McDougall's classification is a useful one. But McDougall is not content with such a descriptive account. Instincts become explanatory units. They are the prime movers of all human activity, reservoirs of force, perhaps mendelian units. The objection is not so much that McDougall has failed to weigh sufficiently the evidence that such complex structures as his propensities are largely developed through experience and that experimentation and detailed observation fail to show any such distinct behaviour patterns as innate mechanisms. Nor are we so much concerned about the criticisms on points of detail, e.g., the number of instincts, the relationship between the cognitive, affective and conative aspects. The final objection is that an instinct as an entity is a mystical conception. How can we represent in observed behaviour, how can we give empirical definition to such a conception as "an innate disposition which determines the organism to perceive (to pay attention to) any object of a certain class, and to experience in its presence a certain

emotional excitement and an impulse to action which finds expression in a specific mode of behaviour in relation to that object"?[1] There is much excellent descriptive work in McDougall, but his quest for ultimate units is just another example of an over-enthusiastic attempt at psychological systematisation.

One last example: In the long argument between Spearman and Thomson the descriptive usefulness of the general factor "g" as conveniently summarising the observed results of mental tests is obscured by the question as to whether it is a final principle of explanation or not. Thus Thomson contends that the supposed common factor can be accounted for by the chance sampling of a very large number of small components. He has shown that statistically this is an equally possible, and, he is inclined to think, a more probable interpretation. He asks: "What are these numerous small components of which mental activities are samples?"; and answers: "Well, I would like to say, in the first place, I do not think they will be in the real mind anything like as separate and discrete as they are represented in mathematical treatment. They are, I think, such things as inherited reflex actions, pieces of behaviour, whether inborn or acquired, memories and a host of experiences, but things we already know of in other connections, and which are in the real mind, not separate elements but the molecular components of a molten alloy in rich solution."[2]

Confronted with this my sympathies are with Spearman! His "g" certainly appears a valuable descriptive concept for just this state of being "molecular components of a molten alloy", for just the fact of integration of capacities which Thomson recognises. More than this, it gives a means of precise statement of the degree of such integration. But is Spearman content with this? No, he goes on to speak of "g" as amount of available mental energy. Again, description is not enough, "g" becomes a cause.

[1] McDougall, W.: "Outline of Psychology", p. 109.

[2] Thomson, G. H.: "Complete Families of Correlation Coefficients", *B.J.P.*, July, 1935.

What shall we conclude then about the problem of analysis in psychology? Psychology, of course, must analyse; its problem is still what goes with what. But how should it analyse? My answer is that it would do better to content itself, for the time being at any rate, with analysis at a descriptive level, and by descriptive level I mean analysis only to such elements as can be given precise empirical definition in the data themselves. Attempts to go beyond this seem to have been more a hindrance than a help.

Does this mean that psychology cannot be a science ordering its data in terms of fundamental laws: as Joseph puts it,[1] "not a science but a collection of more or less detached inquiries"? Well, I think that we are unduly thin-skinned about whether we are scientists or not. Even if we are restricted to detached inquiries, the question is whether they are worth while carrying out. I have no doubt on that score. Nor, despite the tenor of this paper, do I feel unduly humble about the achievements of psychology. It is after all remarkable that despite the warfare of the schools there is a growing body of well attested psychological knowledge. Perhaps the inquiries are not so detached after all. The exponents of schools are naturally inclined to emphasise difference more than agreement, yet there is really a surprising amount in common between them. In effect the differences arise seldom from the actual results of observation and experiment, but generally from the attempts to fit these into a final explanatory framework. It is just the objection to this premature finality that I am arguing. I have perhaps overstated the case and implied a hostility to what is after all a necessity of any study, the attempt to systematise. I have perhaps implied too strongly a flabby eclecticism. Nevertheless, I am convinced of the necessity for psychology to keep free from any premature systematisation (and at this stage any all-comprehensive systematisation is premature) however plausible the scheme may be. Psychology has suffered too much from zealous attempts to fit it into this or that strait-jacket in the quite sincere belief that it was the only suit for it.

[1] *Mind*, XIX, 1910.

MENTAL TESTS IN CLINICAL PRACTICE.

By D. R. Martin.

This article does not intend to make a survey of all the varieties of mental tests that a Child Guidance Clinic may select for its use, for there are many fine expositions in that field.[1] What I want to do is to discuss the purpose of mental testing in child guidance work, and then to describe the particular battery of tests in use at the Clinic to which I am attached, with some reference to the controversy over the value of the Binet test.

The main justification for using any mental test is that it gives us desirable information that could not as easily or quickly be obtained by other methods. In the investigation of the child referred to the Clinic, whether it be for anti-social behaviour, personality deviation, backwardness or what not, a number of different techniques may be utilised. Any method is valuable which will enable us to get accurate information about the child's history, his present difficulty, his heredity and environment, and his physical and mental make-up.

Mental tests which are reliable (i.e which give like performances with equivalent forms or for repeated administrations of the same test, after allowing for practice effects and function fluctuation) and which are valid (i.e. whose results correlate with other criteria or indicators of the trait or ability being measured) will be included among the techniques for studying the child's mentality.

In clinical practice the most widely used of such mental tests are those of general intelligence. Since these tests (e.g. the Binet, the Otis tests, the General Test H, etc.) correlate

[1] Cf. such works as those of Burt, Cattell, Wells, Bronner and Healy, and Garrett and Schneck.

highly with school achievement, they may be regarded as measuring academic ability or indicating potentialities in that direction, and can be used to forecast the child's likelihood of success at school. If there is a marked discrepancy between a child's standing on an intelligence test and his school results, inquiry will seek to elicit the cause: broken schooling, frequent changes of schools, lack of interest in school work, poor teaching, special disabilities, mental conflict, familial upset and the like. The general intelligence test is also useful in the prediction of vocational success, since it has been established that there are certain optimum levels of general ability required for success in various vocations.

Most tests of general intelligence call for an understanding of the written or spoken language, a requirement which is justified by the fact that 'among children having ordinary schooling the "g" saturation of verbal tests is particularly high'.[1] There are also performance tests of intelligence for cases such as the deaf, foreign or illiterate who have not this knowledge of language. If, in the main, tests of general intelligence assume a limited equality of opportunity to acquire skills and experiences which is not always fulfilled, performance tests depend less on such opportunities, and can be used in the case of individuals from other cultures or from underprivileged areas. In particular, they can be administered where, for whatever reason, the child has not had the chance of regular schooling, or where the child's environment has been culturally poor. The performance test of intelligence (such as the Grace Arthur Point Scale of Performance Tests[2]) usually gives a result very close to that of the Binet, but it has been noticed with a group of poor readers coming to the Clinic that the Arthur I.Q. may be anything up to 20 odd points above the Binet I.Q. (obtained with the Revised Stanford-Binet Scales). Such results as

[1] R. B. Cattell, "Measurement versus Intuition in Applied Psychology"; *Character and Personality*. Vol. 6, 1937.

[2] Grace Arthur, *A Point Scale of Performance Tests* (The Commonwealth Fund, New York, 1930).

these give evidence, if any were needed, of the verbal loading of the Binet test.

The work of the mental tester at the Child Guidance Clinic is far from completed with the administration and interpretation of a general intelligence test. It is quite as important to know whether the child has any special abilities or disabilities. In this connection the writer is in the fortunate position of being able to refer children of school leaving age or older to the Vocational Guidance Bureau of the Department of Labour and Industry (in N.S.W.). With younger children it is not the custom to make a detailed study of such special aptitudes as musical, artistic, and mechanical ability and the like, owing to lack of time and material, but such investigation is not unusual in clinics abroad. However, in all cases an attempt is made to assess practical ability as well as scholastic ability. The value of such additional testing is exemplified in the case of a truant of average academic ability and very superior practical ability. Most truants are below average in school attainments and seek a means of escape from an unpleasant situation. In the case mentioned, it seemed that the regular curriculum was not catering for his practical ability and interests which found scope in out of school activities.

A further line of inquiry in all cases of school age is followed by means of standardised tests of scholastic achievement. If any specific disabilities are found in this area, diagnostic tests can be given to locate the child's errors. From a study of the profile of errors obtained by analysing the results of a diagnostic test, and from the nature of the errors made, it may be possible to trace the cause of the disability and to initiate remedial teaching to overcome the weakness. For example, where the reading level of the child is greatly below his achievement in non-verbal subjects (particularly Arithmetic) and below his mental maturity (as measured by general intelligence tests) we have what is known as a specific reading disability. These are not rare phenomena, and they may vary from mere backwardness to total reading

incapacity. And so when a child seems backward in reading, it is important to know whether this is a weakness confined to that field of his activities, or whether it is due to dull intelligence and is only one symptom of general retardation. It might be expected that school results would show its specificity without any need for clinical testing, but school results are not always as reliable as they might be, and in cases where it has been reported that a child is a poor reader but is doing average work in Arithmetic, it has been discovered on administering standardised tests that his Arithmetic level is much the same as his reading status, and that in fact the child is generally retarded. If a child is a 'non-reader' (i.e. has a specific reading disability) such tests as the Monroe Diagnostic Reading Examination[1] can be given to find out just what errors the child falls into most, whether the tendency is to make reversals (i.e. read *was* for *saw*, *from* for *form*, etc.), to misinterpret vowels or consonants, to substitute other words of like configuration or meaning, or what not.

In addition to the investigation of the child's general ability, his special abilities or disabilities, and his school attainments, tests can be used to help to determine his social adjustment, his emotionality, his interests and his attitudes. This is a field in which there are as yet few reliable and valid tests, and in which the clinical programme is comparatively weak. Probably the personal interview is still the most satisfactory method of approach, but it can be supplemented by such devices as the Personal Inventory (or Psychoneurotic Inventory), Word Association tests, tests of fluency and perseveration, and the Rorschach Psychodiagnostik (Ink-Blot) Test. The lastmentioned is still largely experimental, but appears to have been used with success in Europe and America.

It has been claimed (e.g. by Porteus for his Mazes) that performance tests are valuable not only as measures of intelligence, but also because they indicate the subject's

[1] Marion Monroe, *Children Who Cannot Read* (University of Chicago Press, 1932).

initiative, foresight, persistence and emotional stability. Thus Grace Arthur (op. cit.) says 'the performance scale provides an excellent opportunity to observe the behaviour of patients in a variety of situations. Sometimes we find a slow change of "mental set", resulting in slow adjustment to new situations. . . . Other characteristics of behaviour that are most readily observed . . . are a tendency to persevere in the face of difficulty, or to give up too easily; to conform to the rules of the game or to try to "get by" with slight irregularities; to stammer in some situations and not in others.' While bearing in mind that the child's reaction to clinical situations will not be exactly reproduced in other places, and that 'there is no proof that the qualities of persistence or impulsiveness shown in these miniature test situations will throw light on his persistence or impulsiveness in the major situations arising in school and home' (Cattell, op. cit.), it is difficult to believe that the energy or lethargy, careful foresight or impulsiveness, anxiety or stolidity displayed is utterly unconnected with the child's behaviour elsewhere. In all probability the majority of children coming to the clinic will make a showing on these new and interesting tests rather like their behaviour elsewhere, perhaps less obscure, because less overshadowed by the stresses of the school and home situation. Treated as supplementary evidence, then, the performance test can be taken, together with observation of the child's behaviour anywhere else, as indicative of personality characteristics. This evidence will be compared with that obtained from the personal interview and from the more thoroughly standardised personality tests.

Having indicated the main fields of the usefulness of mental tests in clinical practice, and in part their justification, I shall elaborate some of the points touched on earlier in discussing some of the individual and group tests in use at a particular Child Guidance Clinic.

The most time-consuming and most regularly used test is the Revised Stanford-Binet.[1] The English edition of the Scales

[1] Lewis M. Terman and Maud A. Merrill, *Measuring Intelligence* (George G. Harrap and Co., 1937).

is very similar to the American, and as yet the test has not been standardised for Australian use, so some of the tests are probably misplaced in difficulty. As far as the cases tested during my experience with the Scales are concerned only a few alterations would be indicated. Tests that seem too difficult for the years in which they are placed are Similarities: Two things at Year VII, and Similarities and Differences at Year VIII, and ones that seem too easy are Abstract Words II and Picture Absurdities III at Year XIV and Arithmetical Reasoning at the Average Adult level. These are tests in Form L of the Scales which is generally used. The international situation seems to have affected the value of the Abstract Words II test which asks for the meaning of *constant, courage, charity* and *defend.* Warlike definitions of *courage* and *defend* are most frequent, as with the definition of *conquer* in Abstract Words I. The Revised Stanford-Binet Scales are in general a better test than the original Stanford-Binet, and meet many of the objections urged against that test. For example, it has been extended at both ends of the scale, and now tests as low as Year II and as high as three Superior Adult levels. There are two equivalent Forms of the test, and although Form L seems preferable to Form M, since it alone contains the Vocabulary Test which Terman and Merrill describe as 'the most valuable single test in the scale', Form M is a useful check test. Certain undesirable tests have been dropped such as giving age, sex, name and surname, giving the date, and naming coins. The directions for administration and scoring are more explicit, leaving less to the examiner's discretion.

Some objections to the Binet still remain, and will always remain as long as the test is at all like the original Binet, and since this is the test most used in clinical practice, some consideration should be given them. They strike at the root of the two main advantages claimed for the Binet. Now, the chief virtues of this test are said to be (a) that the individual test situation can be more fully controlled than the group test situation, and that the investigator can and should make

certain by suitable motivation that the subject is doing his best; and (b) that the method of sampling a number of abilities in the Binet gives a better picture in less time of the subject's personality as a whole than do group tests.

Against advantage (a) it has been pointed out by R. B. Cattell (op. cit.) that the 'personal relationship which arises between tester and testee . . . is just as likely to introduce errors arising from shyness and other temperamental variations in the child' (e.g. such as contra-suggestibility), and the 'halo' effect may operate. However, it can usually be seen when for personal reasons the test is failing in its object of measurement, and if in such cases the test is abandoned, or is supplemented by other tests, the danger of obtaining an assessment of mental maturity too high or too low will be lessened. Indeed, whatever the test being used to measure intelligence, supplementation is necessary out of fairness to the subject being tested. A couple of examples may make this contention clearer: In one case, a seasoned delinquent throughout the giving of the Binet tried to provoke the tester's anger, and consistently refused to attempt tests that looked at all difficult. Her performance on the Otis (A.C.E.R. adaptation) which she administered to herself away from all social contact was considerably better than the Binet would have indicated taken at face value. In the other case, a schoolgirl of careful and pedantic habits of thought had spent the full time allowed on the Otis doing about half the items, making no mistakes, but obtaining a score only a little above the average. On the Binet, however, given a patient hearing and plenty of time she obtained a superior rating. These examples show the advantage of always giving several intelligence tests of individual and group variety.

Against advantage (b) Cattell points out that 'if the test is not concerned with any one ability but a collection of abilities, the attachment of a single quantitative value to this hodge podge is meaningless'. P. E. Vernon, however, indicates that the 'summation of heterogeneous items may give a moderately efficient measure of *g*, because the specific

factors tend to cancel one another'.[1] In this connection it must be remembered that although the test samples a number of abilities, the habit of analysing the Binet performance, and, for example, saying that a child's memory is good or his practical ability poor, on the basis of success or failure in different sections of the test, is as yet unjustified since the Binet has not been subjected to factor analysis.

It would seem that we must accept the Binet for what it is, namely a valid measure of scholastic ability or potentiality, and not expect too much from the instrument, although we can with careful observation glean some information about the child's personality from his reactions during the test. In view of the caution necessary to observe with any intelligence test (not only the Binet) the fact that results are so frequently given in points of I.Q. has led to an illusion of accuracy, particularly in the layman's mind. It is expected by some people, for example, that a child with an I.Q. of 96 should be perceptibly brighter than a child with, say, an I.Q. of 93 or 94. Such an expectation does not take into account the fact that the P.E. of the I.Q. in this region of the Revised Stanford-Binet Scales is 3·04, which means that 'the chances are even that a score which falls in the I.Q. range 90 to 109 does not differ from the true score on such a scale by more than 3 points and the chances are five to one that it does not differ from the true score by more than 6 points, or twenty-two to one that it is not in error by more than 9 points'.[2] This shows how unjustified it is to expect too much from small differences of I.Q. obtained from such a test, and since other tests of intelligence are in the main less reliable than the Binet, how much more caution is necessary in using such results. However, as long as it is the custom to express results in terms of I.Q. (and it is now traditional) we can expect the illusion of accuracy to persist.

Along with the Revised Stanford-Binet, in trying to sample as widely as possible the field of the child's abilities,

[1] P. E. Vernon, "The Stanford-Binet Test as a Psychometric Method"; *Character and Personality*, Vol. 6, 1937.

[2] Terman and Merrill, op. cit.

the Clinic uses the Otis tests of which there are several forms at both Higher and Intermediate levels adapted by the Australian Council for Educational Research; the A.C.E.R. Non-Verbal Tests; the Grace Arthur Performance Scales; and the Alexander Performance Scale. Not all of these will be given to the same child, because of lack of time, and because their suitability is limited to various age levels. For example, while the Arthur Performance Scale (including such tests as the Sèguin Form Board, the Manikin, Healy Picture Completion Test, Porteus Mazes and other similar tests) is suitable for children 5 to 12 years of age, the Alexander Performance Scale is best used above the age of 11. This Scale[1] consists of the Passalong Test, an abbreviated Kohs Block Design Test, and the Cube Construction Test. Of these the Passalong is a new kind of performance test, consisting of blue and red pieces which must be moved in a box, without lifting them out, so as to arrive at a reversed pattern. The early patterns are easy, the later ones more and more difficult. This is a test the child finds interesting, and further research may show its usefulness as a means of investigating personality as well as of assessing practical ability. Among those tested two extreme types have been observed: those who make many quick impulsive moves or 'trials' while they are solving the problem, and those who hesitate to make a move before they give it long consideration. The bulk of cases tends to lie between these extremes.

In the investigation of the child's school achievements the Clinic has at its disposal the A.C.E.R. tests of Silent Reading, Arithmetic, and Spelling, and the A.C.E.R. Individual Reading Test for those cases not suitable for the Silent Reading Test. If the child is referred as a possible 'non-reader', Gray's Oral Reading Paragraphs and the Marion Monroe Diagnostic Reading Examination help to locate the child's errors and can be used as a basis for remedial teaching. Reading disability cases are also given a battery of handedness and eyedness tests to determine lateral

[1] W. P. Alexander, *Intelligence, Concrete and Abstract* (Cambridge University Press).

dominance, since this is sometimes a factor in the difficulty of orientation revealed in reversal errors.

The personality test in most constant use at the Clinic is the Personal Inventory, devised by Dr. A. H. Martin (of Sydney University) or an amended version made by Mr. D. E. Rose of the Vocational Guidance Bureau. The Personal Inventory is a successful indicator of emotional stability and introversive trends, and can be used as the basis of further inquiry into the child's personality during an interview. The Rorschach Psychodiagnostik Test is included in the Clinic's equipment, but so far lack of time has limited its use. It is a test needing time both for administration, and for analysis and interpretation. Where three hours is usually the outside limit of the time devoted to mental testing each individual, the Rorschach Method cannot often be fitted in. Further research into its usefulness is thus needed.

In the various ways discussed, the psychological investigation at the Clinic tries to make a survey by means of reliable and valid tests of the child's general intelligence, his special abilities and disabilities, his school attainments, his personality traits and emotional stability. The results gained help in the diagnosis of the child's difficulty, and in treatment suggested. Such diagnosis and prognosis can only be attempted when it is seen what rôle is played by the child's abilities and traits in his life history.

REVIEWS.

PSYCHOLOGY FOR EVERYONE: AN OUTLINE OF GENERAL PSYCHOLOGY. By W. J. H. Sprott. Longmans, Green and Co. London, 1937. Pp. 446. Price: 8s. 6d.

Intending this book for the "general public", the author declares himself an adherent of no "school" of psychology. "We shall find", he says (preface, p. ix), "that there is much to be said for all theories, *with respect to the areas which they cover*, but that so far there is no grand theory which covers the whole field of enquiry." It is, of course, desirable that those who are approaching a subject should not be presented with any dogmatic system, that they should be given to understand that the matters dealt with are controversial and that they should be shown what form some of the controversies take. But it is especially the beginner who will be thrown into confusion if he is confronted with competing principles but no definite position; and it is only such a position that will enable him to see "what the varying authorities are up to, why they differ, and what their differences signify". For the critical student an account of what "psychologists" are doing —incidentally indicating how uncritical much of their procedure is—will be of considerable interest, even if it is not psychology; but it is of little use to the beginner, for whom the subject itself has yet to take shape. It is not to be disputed that Sprott raises many real questions and makes useful points in regard to them; but they do not cohere, and the discussion of them is submerged in a sea of "views".

In fact, one might say that the book is of a "put together" character, and there are many minor indications of hastiness in the putting together. Apart from the considerable number of misprints (e.g., "libidual" for libidinal on p. 137, "wrecking" for wreaking on p. 163, and hedonic "tome" in several places), there are many instances of the mixing up of singulars and plurals; and those who, in this connection, have wearied of

protesting against the use of "data" with a singular verb, would hardly take "phenomena" (pp. 172, 409) to be in like case. "Interpretive", again, might seem to have acquired a certain sanction (though Sprott uses "interpretative" at least once), but what is one to say about such words as "structured", "structurated", "decentration" as opposed to concentration, and "lovee" (pp. 15, 191) as correlative to lover? There are also many cases of the misuse of words, e.g., "detraction" (p. 107) as the opposite of attraction, "enormity" (p. 164) in the sense of great extent, and "hypothecate" (p. 206) for hypostatise. And, while the exposition on the whole is clear enough, there is a good deal of loose writing, of which the following may be taken as samples: "This difference is more concerned with the way in which images function rather than with different kinds of imagery" (p. 327): "Adult sexual life seems more complicated than at first sight appears" (p. 137)—the "seems" here being an illustration of the indecisiveness which appears again in the frequent description of points as "difficult to see" or of doctrines as "going too far", and which, as has been suggested above, is characteristic of the general method ("impartial" presentation) adopted by the author.

The ascription of a certain value to all views, and the connected treatment of theories as *instruments*, are not, of course, peculiar to him or to the class of psychological theorists; they are a notable feature of contemporary "scientific" thought. But, since psychologists are commonly supposed to have special competence, a peculiar authority, on such questions, the exposure of any such pretension is of some importance. The position is stated by Sprott as follows, at the beginning and the end of his book: "Psychological theories are not utterances of absolute truth, they are useful devices for understanding. If a theory ceases to be useful, we cast it aside and turn to another; theories are to be *used* rather than to be believed in" (p. 5), and again: "For practical purposes the psychologist has to use the body-mind framework which he finds the most helpful. His business is to display experience and behaviour, or that special field of experience and behaviour

in which he is interested, as a coherent system, and he has to make use of the constructs 'body' and 'mind'. By means of his explanatory frameworks he displays connections which we have missed, and aspects or possibilities of experience which we have not appreciated. His ideal is to enable us to predict what is likely to happen if certain conditions are fulfilled. In so far as he helps us to understand and foretell, his framework is of value, and whether our success in understanding and foretelling is evidence of the truth of his framework is a matter which we can safely leave to the philosophers to decide" (pp. 436, 7). Now here the first point to consider is how a theory or a framework can be "used" or be "helpful"; actually it can be so only as premises, as things asserted in the very same way as the conclusions to which they lead, asserted, i.e., as *true*. Certainly, true conclusions may follow from premises which are not true, a hypothesis may be verified many times and yet falsified in the end (and, in this connection, Sprott speaks curiously, in the chapter on Belief, of "the inference to a hypothesis"), but, still, the same sort of issue, of truth or falsity, is raised in its assertion as in any other assertion. Secondly, when, here and elsewhere, Sprott passes over questions to the philosophers, it is important to observe that, if the questions have any relevance to psychology, it is the psychologist's business to study them (even if this means that he has to become philosophic) as much as any other questions in the psychological field. He may, of course, adopt a philosophical position without having given it as *thorough* study as the professional philosopher does. But if, without philosophic study, he adopts a theory of "frameworks" or of "usefulness" as contrasted with truth, his procedure is that of a philosophical dogmatist, not of a scientific psychologist.

The fact that true conclusions can be drawn from a number of different theories is not a reason for describing them all as "useful". A theory embodies many propositions, of which some may be true and others false, and the drawing of true conclusions from the former confers no usefulness on the latter or on the theory "as a whole". If theories conflict,

one at least will contain false propositions, from which false conclusions are derivable, and so it will be important to come to a decision as to the truth of the theories or their constituents. Sprott takes the theories of Freud, Adler and Jung as each being useful, but he takes no account of the contention of the Freudians that what is of value in Adler's theory is simply a *part* of their own position. In fact, the distinction between the instrumental and the material parts of any study is just one of those (functional or rationalistic) distinctions which it is the business of philosophical criticism to remove; and, while Sprott does not develop the common theory of "concepts" as instruments, he at least speaks of the formation of concepts, and, in general, distinguishes types of cognition, without consideration of the fact that *any* knowledge is "conceptual", in the sense that we cannot know anything except as of some sort. Again, while presenting quite clearly, and bringing out some of the difficulties of, the theory of conditioned reflexes (in Ch. IV, on Action), he considers that it brings out a principle ("mechanical linkage") of certain of our actions—while, in fact, such actions appear in his exposition as cognitive activity in relation to developing situations, i.e., as of the same sort as *thinking*. A minor instance of conventional and unnecessary distinction is seen in the suggestion (p. 409) that "there might be other means of having experiences" than by getting material "through the sense organs"—as if there were any difference between a means and an organ.

The two important considerations, of mind as active or conational (as having "interests") and of its being confronted with situations, not "data" or elements, appear in a somewhat indefinite form in Sprott's book, though it may be said that the most vigorous and informative parts of it are his discussions of the Freudian and "Gestalt" theories, which bear most directly on these two points. Even in his account of "activist" views he cannot get away from the notion of passive reception. Thus, on p. 203, he contrasts the views that "a human being is a machine responding to stimulus" and

that "what responds is something which you can call a purposive urge"—as if seeking, rather than responding, would not be the characteristic of an urge. Similarly, on p. 277, he gives as a point in favour of Hartshorn's theory of sensations as the "solidified projections of emotional disturbance" (yellow being called a gay colour "not because we feel gay when we see yellow, but because we see yellow when we feel gay") that "the way in which our interests influence our perceptual field, the close connection which we have emphasised between one aspect of organic functioning and all the others, warn us against taking the naive view that our sense organs are merely avenues through which we see and hear the world as it really is: they are rather avenues through which we *receive stimuli which we make use of*, and which affect every part of our being" (my italics)—thus still taking the stimulus as prior to the activity. And when he goes on to say that "the theory of Hartshorn goes too far in the direction of subjectivity; after all, the sad and the gay will not differ from one another in the colour which they will ascribe to a field of marigolds", he misses the point, brought out also in Freudian theory, that (assuming there really is any connection between yellow and gaiety) the sad person may not see the marigolds at all.

The view of mind and its surroundings as alike complex and active would lead to a great reduction in the attention to be paid to sense-mechanisms and cognition generally, i.e., the matters taken up in the latter half of this book would be seen to be relatively unimportant for the psychologist (and, as previously suggested, many current distinctions would be rejected), and the theory of action presented in the earlier part would be correspondingly expanded. At the same time, in the consideration of the "Innate Constitution of Man" (Ch. III), much more would be made of what men have in common, and facile classifications of human types, on the basis of which a mass of futile "testing" goes on, would be abandoned. From this point of view, too, the difficulties attending theories of emotion and of mind and body could be greatly reduced; for, as against any theory, of the currently

abstract kind, of emotional or physiological *accompaniments* of thought and action, there would be no logical barrier to the treatment of what thinks and acts as *itself* both emotional and physiological.

John Anderson.

General and Social Psychology: A Textbook for Students of Economics and of Social Sciences. By Robert H. Thouless. Second ed. London, University Tutorial Press, 1937. Pp. xii, 522. (Aust. price, 14s.)

As the preface of the first edition of this book indicates, it is "an attempt to cover the syllabus of the Psychology subsection of Sociology in the B.Sc. (Economics) Examination of London University". The reader is everywhere made conscious of this aim, and the book may be justly criticised only if the aim is borne in mind. No other explanation may be given for the apparent lack of plan.

That Thouless has covered a wide range of psychological problems may be indicated by the mention of some of the chapter headings. The book begins with a chapter on the Science of Psychology and its Methodology. Chapter II then introduces Innate Pattern Reactions, while Chapters III and IV are given to Acquired Patterns of Behaviour. Then follow discussions of the Emotions, Sentiments, and Character and Personality. Chapters VII to XI revert to the question of Motivation. Chapter XII is given to Perception, Chapters XIII and XIV to Thought and Language; Chapter XV to Conflict and Volition. The next two chapters offer the strictly social aspects of the work. In Chapter XVI are discussions of the Social Behaviour Tendencies, and in Chapter XVII, which is new to this edition, the problem of Social Grouping is given some attention. This new section was added as a result of criticism of the first edition to the effect that the nature of group reactions in society and the class system required treatment in such a text as this. Chapters XVIII to XXV treat special problems in an un-coordinated fashion. Among the topics discussed are Morality, Economic Value,

Statistical Method, Mental Testing, Intelligence, Aesthetics and Religion.

In addition to Chapter XVII, Chapter XXI, on the use of Statistical Method, is new to this edition. In the short space of twenty-five pages Thouless endeavours to present a "satisfactory elementary introduction" to the subject of statistical method. The present reviewer finds in this section a collection of hints to advanced students doing research but a quite unsatisfactory introduction to the subject for beginners, and an insufficient explanation even for the more advanced student. This and the rest of these last eight chapters give the impression that they have been "thrown in". The reviewer seems to detect an uncomfortable striving to cover all the topics of a syllabus and a failure to preserve any logical relation between them.

The former Chapter XVIII has, for example, been expanded into two chapters, XXII and XXIII, "in order to take in the new work on mental testing and factor analysis". Not only has Chapter XXIII a very doubtful place in a beginning text, but it fails to do justice to the factor theories. Again the criticism is one of policy. Would it not be preferable for the teacher of beginning students to choose a path for them to tread rather than to point to a maze of paths and abandon them to their own devices?

Apart, however, from the general form and policy of the text, there is much that is worthy of praise. Many of the chapters, while failing to fit into a logical plan, are creditable in themselves. The book is clearly written, is interesting, and introduces the student to a wide field, and to the literature of that field, very competently.

The author calls attention to a major change in the book in view of the fact that he now uses the term instinct in a more restricted sense than in the earlier edition. The change is more or less equivalent to McDougall's use of "propensity". Thouless believes that the underlying concept should imply less fixed inherited patterns of behaviour in human beings.

One might quarrel, however, with the treatment of Emotion, which leaves the impression that it has not been revised since the first edition of the book in 1925. The James-Lange theory is treated and criticised in traditional style, but no mention is made of such recent research as that by Cannon, Bard and Lashley. Had more space been given to completing those sections vital to the argument and less to scratching the surface of such a field as Factor Analysis, one would feel infinitely happier about the book.

The reader brought up in anything other than the English tradition will find the chapter on Character and Personality difficult to digest. The term Personality is used as equivalent to Individuality, and "a man's 'character' is the kind of personality he has" (p. 112). Every possible quality of personality is included in the discussion, but there is no system. Many of the difficulties encountered in Psychology are terminological, but Thouless does little, here or elsewhere, to clear the atmosphere.

Reviewing this book for the *American Journal of Psychology* (January, 1938), Ruckmick "regrets that books like this one are not more readily available to American students". He also says: "The book is one which the science of Psychology must reckon as among the few best social psychologies that have been written with authority by well-equipped psychologists." If the book may be regarded rather as a psychological treatment of some social problems than as a "social psychology", the present reviewer concurs.

C. A. Gibb.

Critical Realism. by G. Dawes Hicks. Macmillan and Co. Ltd., London, 1938. Pp. 338. Price 15s. net.

With the exception of 'Conceptual Thought and Real Existence', which is a revised and expanded lecture, the essays making up this volume have been published previously, their dates ranging from 1916 to 1934. They appear now in the main as before, but with "some not unimportant corrections", and were selected by Dawes Hicks as working out in detail his

view of the relation of mind to nature. This is not obvious in the case of the last two, *The Philosophical Researches of Meinong* (1922) and *The "Modes" of Spinoza and the "Monads" of Leibniz* (1918). The first of these is an attempt to give an outline of Meinong's ways of thinking which will be of use to those unacquainted with his work, and special attention is paid to the early "Hume-Studien" as presenting Meinong with the problems of knowledge which he tried to meet by his later "Gegenstandstheorie". The influence of this theory on Dawes Hicks's work is apparent in other essays, and he justifies the inclusion of this historical survey on that account. The second is an excellent disposal of the sharp antithesis drawn by Hegel and more recent expositors between the positions of Spinoza and Leibniz, and the place of consciousness in their systems gives the connection with the main problems of the book. In general, however, Dawes Hicks presents his own position by reference to work of the present century, philosophic and scientific; and within this sphere he deals chiefly with philosophical theories of a non-idealist character. Except for the sixth essay, *F. H. Bradley's Treatment of Nature* (1925), he does not concern himself with the idealist view of the relation of mind and nature.

Dawes Hicks accepts as fitting his own standpoint Perry's definition of *Realism* as standing for the principle that "things may be, and are, directly experienced, without owing either their being or their nature to that circumstance", provided no special interpretation be given to "directly experienced". The concentration on knowledge which this definition suggests is defended in the Introduction where Dawes Hicks takes up the question—Can Realism dispense with a theory of knowledge? dealing with Marvin's plea for a return to dogmatism in "The New Realism". The brief discussion by no means does justice to Marvin's article. Not only does Dawes Hicks seize upon unfortunate expressions to charge Marvin with a misinterpretation of Kant which does not appear from the argument as a whole, but of the two points he raises, one amounts to the demand for a prior

criticism of the instrument, the other could quite consistently be made by Marvin. As to the latter, he argues: "Epistemology most assuredly cannot 'give a theory of reality', but any 'theory of reality' will remain a dubious structure, until the conceptions that have been used in framing it have been examined and tested" (p. xxii). At this point he mentions in a footnote Marvin's inclusion of the study of the logical foundations of the sciences under the term metaphysics, and remarks that he cannot see what this subject can consist of, if not of the problems he has been indicating. These problems may be summed up as the possibility of misapplying categories in the study of particular fields of facts. And whether or no this is precisely what Marvin meant, it is clear that he would not see these as problems of epistemology. Dawes Hicks says that "whether the group of problems which thus arise be described as epistemological, logical or metaphysical, is a matter of small import" (p. xxiv), but unless the examination and testing of these conceptions be conceived as epistemological in Marvin's sense, Dawes Hicks's statement has no bearing on his thesis. Dawes Hicks's position is very close to Kant's; he holds that conceptions can be extended so as to cover fields of reality to which they do not apply. But unless his position has changed since the writing of the first essay, *The Basis of Critical Realism* (1917), he disagrees with Kant at what is just the vital point in relation to Marvin's argument. For Kant held that these conceptions (cause and effect etc.) were "forms of thought" which had the function of synthesising, whereas in that essay, Dawes Hicks emphatically opposes to this his own view of knowledge as "an act of *discriminating,* of *distinguishing,* of *comparing* features which, as presented, are already synthesised" (p. 7). Further, Dawes Hicks seems not to grasp what Marvin means by "a theory of reality", but to understand this as "the philosophical treatment of any order of facts—whether those of outer nature or those of the mental life—" (p. xxii), which means that he makes nothing of Marvin's point that any theory of knowledge *presupposes* a theory of reality.

The thesis of the first five essays is that *discrimination* is the essence of any act of apprehension, however primitive and crude that act may be. Two important implications of this theory are that there is "no cognitive relation in regard to which the question of truth or error cannot arise" (p. 24) and that so-called "sense-data" are not "detached elements affecting the mind", but "qualities or properties of physical things" (p. 19). The latter, "common-sense" view is upheld against Russell in the first essay, and against Broad in essays II and III, *The Sensum Theory* (1916) and *Sensible Appearances and Material Things* (1922). Dawes Hicks justly demands of theories which reject the common-sense view that they account for its existence. This question is disposed of by Russell by the remark that the common-sense world of ordinary experience has been built up by "our savage ancestors in some very remote prehistoric epoch", by a "piece of audacious metaphysical theorising". And Dawes Hicks argues that if Russell undertook to show the manner of this building up, he would have to follow the Kantian method, because common-sense experience contains conceptual elements which are not part of the given, and so must be introduced into it by the apprehending mind. He would have to conceive of thought as "an instrument by means of which the crude data of sense are worked up into the form of knowledge" (p. 23). And the objection to such a theory is the postulation of "a unique faculty of thinking or judging, suddenly emerging full-fledged into being, possessed from the very start with a whole apparatus of categories, and concerning which all we can do is to point to the results to which, ex hypothesi, it gives rise" (p. 72).

Whereas Russell presents "things" as merely "logical constructions" or "symbolic fictions", Broad combines the theory that what we directly perceive are sensa (existents which are neither physical nor mental), with the belief that there are physical things possessing qualities, which things or qualities either are or seem to be manifested to us by sensa. Against such a theory Dawes Hicks can make the points,

familiar as criticisms of Locke, that it is impossible to prove that physical objects exist (this being admitted by Broad), and impossible also to show why sensa should disclose to us the shapes, sizes and positions of physical objects, when they are held to delude us concerning secondary qualities. Nor could Broad show how "the shapes, sizes and positions of visual or tactual sensa, as they occur in visible or tactual space, could exhibit the shapes, sizes and positions of things that are not in visible or tactual space, and whose structure is confessedly quite different from the structure of sensa" (p. 54-5). But the main point made against Broad is that in his endeavour to explain how it is that we come to ascribe qualities, not to sensa, but immediately to physical things, he is unable to present a consistent view of the process of perceiving. On the one hand he contends that what we directly apprehend in every perceptual situation are the qualities of sensa, on the other hand that we use sensa mostly as signs and do not notice their qualities or properties, in a way analogous to our use of printed words on a page. As Dawes Hicks argues—"The most that we should be entitled to assert would be that we do not usually recognise that they are qualities or properties of *sensa*" (p. 57). Connected with this is the argument that sense-data or sensa cannot be taken as infallible "appearances", but that in regard to them appearance, as understood by common-sense, breaks out again. Thus Broad would admit that at the purely perceptual level people do not "judge" that a sensum is a part of an external thing, but "intuitively" take it to be such a part, and so, on Broad's theory of the sensum, are perceiving it falsely (p. 60). Against Russell's theory that a so-called thing is not a single persistent entity, but a series of entities succeeding each other in time and each lasting for a very brief period, he points out that when he is "in immediate relation to that complex of sense-data which, ex hypothesi, constitute what he calls the table", this appears to persist for the very considerable time during which he is looking at it. That is, this cinematographic illusion occurs when he should have that knowledge by

acquaintance which Russell presented as not open to illusion (p. 29-30).

A central part of Broad's general position is his theory of "sense-fields" and "sense-histories", involving the contention that special occurrent conditions (e.g. visual and auditory stimulations) do not produce special acts of sensing, as Dawes Hicks holds, but rather produce outstanding sensa in our special sense-histories. Instead of the special acts there is a general process of sensing, and for this a continual series of internal sensa, as objects. Dawes Hicks is concerned with Broad's view of the general process of sensing, and its precise relation to "feeling", but while this is a logical point of attack, he confuses his argument by discovering "a curious inconsistency" in Broad's use of the term "feeling" which only appears if isolated sentences are taken without reference to the context. In "Scientific Thought" it is not the case that Broad "*strenuously insists* that the terms 'sensation' and 'feeling' indicate two quite different kinds of experience, one of which can and the other of which cannot be analysed into act and object" (p. 62, my emphasis). Actually he is rather tentative about the relation between sensation and feeling, being definite only on the point that "some sensations at least are analysable into act of sensing and sensum, and therefore that we cannot argue that sensum = sensation = a presentation" (p. 257). The rule which he gives (p. 255) is one concerning the distinction made by *language*—"when a sensuous experience seems clearly to involve act and object, it is called a sensation and never a feeling; when it is doubtful whether any such analysis can be applied, it is called indifferently a feeling or a sensation". And the contention, referred to by Dawes Hicks, that "we talk of a sensation of red, but never of a feeling of red, or of a red feeling", is given as an example of linguistic usage. Nevertheless from this and a later part of the book on "The Conditions and Status of Sensa", it appears that Broad does *tentatively* accept the distinction as it appears in language and take a feeling as a presentation. Further, in "The Mind and Its Place in Nature"

(p. 307) he does present feeling along with sensation as "a mode of cognition". But in view of the tentative nature of the distinction made between the two in "Scientific Thought", it hardly seems correct to find an inconsistency here, if indeed one should concern oneself with inconsistencies between two different books, unless the writer continues in the second a general line of argument which *depends* on a theory presented in the first and rejected, or in some way inconsistent with theories, in the second. That Broad does this does not seem to be the case, and Dawes Hicks certainly does not show it. All that he shows along such lines is that a view of the general process of sensing put forward in an extremely tentative way in "Scientific Thought", which takes this general process as the somatic sense-history, and presents this as unanalysable into act and object, is incompatible with the view which Broad takes of "feeling" or "bodily sensation" in "The Mind and Its Place in Nature". In the latter work Broad, though still expressing himself in undogmatic fashion, appears to reject the view that "feelings" are simply presentations, and to argue that a toothache and a noise are on the same footing, that they are *objects,* i.e. that we can distinguish between the existence of toothaches and a feeling of them, just as much as we can between the existence of noises and the sensing of them. Dawes Hicks himself rejects this theory—"we do not apprehend in and through feeling; what is felt is never regarded by us as an attribute or quality of an object" (p. 62). Feeling, he argues, involves no distinguishing, comparing, relating, which he takes as necessary to knowledge, but is rather "a mode of being affected" (p. 63). Allowing for the ambiguity of the word "feeling" each of these statements could be questioned, and Broad himself gives arguments which Dawes Hicks does not consider, for the view that feeling, as understood by him, is a case of knowing. And since the study of cognition has such a central place in the book, a more detailed consideration of such a view would be welcome.

The identification of the "sensible object" with the "physical object" which Dawes Hicks opposes to theories of

sense-data or sensa is based on the theory that a thing or object is "enormously complex" (p. 31). He speaks of the *sum* of features characterising the object, but argues that we have only limited powers of discrimination so that the sum of qualities which we actually discriminate is always different from this (p. 75). There is always more than we can discern, but "there is no ground for supposing that the 'more' in the object is inconsistent with the features that are perceptible. Under the 'more' may quite well be included the elements which the physicist has good reason for thinking go to constitute the 'matter' of the object" (p. 31). An answer could be given, along these lines, to the appeal to the "scientific" view of things as something quite distinct from ordinary experience, which is one of the arguments for the existence of sense-data. As Dawes Hicks expresses it: "A luminous body may *both* shine with a red light *and* consist of particles vibrating at the rate, roughly, of four hundred billion times a second" (p. 31). Theories of sense-data also appeal to the fact that what is called the one object appears differently to different observers, and to the same observer under different conditions, and Dawes Hicks tries to meet this by a special theory of "appearances". These, he argues, are not existing entities. "A 'thing' is made up of parts and of qualities, and any one of its qualities may 'appear' in a countless number of ways. But this quality is not resolvable into its ways of appearing; it remains one though its appearances are many, and is, as such, a quality of the 'real thing', while the appearances of it are not. The appearances are no more than the orderly manner in which the quality is apprehended by a finite mind under the conditions and limitations imposed by sense-intuition" (p. 46).

This theory is elaborated in the third and fourth essays. As appears especially on pages 74 and 75 Dawes Hicks uses the term "appearance" to cover true as well as false knowledge. "Always in sense-perception there *must* arise the contrast between what I have called the content apprehended and the content of the actual object upon which, as I take it,

the act of apprehension is directed." "In contradistinction to the richness, the fulness, the completeness of a really existent object, the term 'appearance' or 'phenomenon' carries with it, therefore, the significance of fragmentariness, of incompleteness, of mutilation—peculiarities that point to the abstraction characteristic of apprehension in all its stages." Restricting the case to one of true knowledge of an object, and on the basis of the theory that there is always "more" in the object uncognised but not inconsistent with what has been discovered, it seems pointless to call what has been discovered an appearance of the object, especially if that term is to suggest mutilation and incompleteness. Mutilation and truth are strange companions, and while the facts discovered are not anything that could be called the "complete truth" about the object, the theory of the ever-present "more" surely amounted to the recognition that this was impossible, and these facts would still be "completely" true. Dawes Hicks rejects "appearances" when these are taken, as by Broad, as "existent" entities, but he accepts a distinction made by him between "abstracta" and "existents". His contention is that sensible appearances are such abstracta, because they, "when regarded in and for themselves, are just qualities or complexes of qualities, . . . they are real but not existents" (p. 77). They "neither are nor appear to be directly and literally in time", they are not, strictly speaking, in space. They do not act and react upon each other, but are outside the realm of temporal flux and change (p. 77). The "content apprehended" is another term for the appearance of an object, and concerning it Dawes Hicks says it "cannot itself persist after the act through and by means of which it has its being has ceased to exist" (p. 94). While, then, he denies that any *object* known depends for its being on its being known, he makes these appearances "owe their being" to the circumstance that they are known (see quotation from Perry).

Appearances are essentially *apprehended* qualities, but it is because they are qualities that Dawes Hicks calls them abstracta, and in the antithesis drawn between things or

existents and qualities lies the real weakness of the theory. And this emerges from consideration of Broad's objection to the theory. Broad takes mirror-images and contends that Dawes Hicks must either hold that a single extended particular can appear to be two extended particulars at a distance apart from one another, or that sensible qualities and forms appear to inhere not in physical objects but in regions of space. This second view he argues presupposes absolute space-time as a substantial matrix in whose regions qualities can appear to inhere. Dawes Hicks takes up the second suggestion, arguing that the space behind the mirror where the sensible qualities appear to be, must contain at least air and the innumerable particles which air contains, and that this would provide substance enough for sensible qualities to inhere in or to appear to inhere in (p. 52). First as regards Broad, the mirror-image does not raise peculiar difficulties in respect of space and time. We are constantly and quite correctly making statements of the kind—such and such qualities are at a certain distance behind another thing. That is, we are constantly referring qualities simply to a certain point in space, to a certain place, designated by its relation to things about it. And such statements are of the same general character as those in which, as it is said, a quality is predicated of a thing or existent. When we say—This table is round, we already know some of the characters occurring at a certain place, those covered by the term "table", and we observe another. In the case of such a statement as—There is a table five feet behind me, I may be discovering the presence of qualities in that place, five feet behind me, for the first time. Secondly, Dawes Hicks's calling in of air seems special pleading, and in any case air would be as much a quality actually found in that place as the qualities which only appear to be there. Dawes Hicks could not show what an 'existent' or thing was, when conceived in abstraction from all qualities, except a mere place. And unless we recognise that we are never acquainted with mere abstract places on the one hand, and mere abstract qualities on the other, we cannot

explain how we come to "place" qualities as we do. Whether or no there be air present at the given place behind the mirror where the other qualities appear to be, what we observe when we examine the mirror-image contains no reference to air, but is of the form—At a place at such a distance behind the mirror are the qualities xyz. Actually those qualities do not exist there, possibly the quality air does, but there is no need, except for a theory which sets up existents as necessary foundations for the presence or appearance of qualities, for a theory of knowledge to specify qualities which do exist there. But Dawes Hicks is in the position of having to find some actual "thing" which he can then say, "appears" in these ways to the observers.

Important as Dawes Hicks's insistence on Discrimination is, as against theories of Sensa, it needs restating. His setting up of a special class of "reals", namely appearances, shows that he is not free from the tendency to make relations constitutive, the rejection of which by the New Realism (see Marvin—Program and Platform of Six Realists: "The New Realism", pp. 473-4) is a feature of the theory to which Dawes Hicks might have turned, rather than to the particular theory of knowledge which Holt set out in "The Concept of Consciousness" and which is criticised in the first essay.

The need for a recognition that knowledge is a relation comes out even more clearly from his treatment of mind in the first essay. He argues that: "Every act of awareness is a specific concrete act, and its specific character is determined or partially so by what it is awareness of" (p. 15). If by "act of awareness" were meant here a whole complex *A knowing B*, then there is a difference of quality between two such complex situations taken as wholes, between the knowing of blue and the knowing of a primrose, owing to the difference of one of the elements in the two situations. But this is not at all what is meant. Dawes Hicks is ready (although only partially, as is seen in the theory of appearances) to distinguish an object B from its being known, but he does not recognise a thing A which is distinct from its knowing. He accepts Meinong's

theory that mental events cannot be described in and for themselves as can physical events, and necessarily so, since he takes as mental events "acts of knowing" or of "apprehending". And the result of this is that he is compelled to take the object known into the mental event, and thus does away with its independence. Using the term "content" to denote the specific character of an act of awareness, he says: "Awareness of hardness . . . is a content of mind, but awareness of hardness is not a compound made up of the constituents, awareness and hardness" (p. 17). We are to conceive of a physical object possessing the quality hardness, and also of a mental event, "awareness of hardness", by which the content of the physical object is apprehended. We may agree that we cannot split up a situation into awareness and hardness, in the sense that we cannot abstract something which we can call awareness, any more than we can abstract "on-ness" or "besideness". This shows that what we mean by relations are not things which "fall between" one term and another. But we can distinguish a term from the whole situation, can consider its nature apart from its relation to the other term, even in the cases where that nature would be different if it were not in that particular relation. We can consider hardness, or rather the particular hard thing, by itself, though this certainly does not leave us with "awareness"—it leaves us with another thing related to that hard object. The reason why we cannot find constituents of "awareness of hardness" is that this is an incomplete expression, not denoting any actual state of affairs, unless we understand as included under it the other term of the relation; and if we do that we can find the two terms as constituents, although we still cannot isolate a third thing "awareness". By "awareness of hardness", Dawes Hicks urges, he does not mean any sort of representation of the object, but when he affirms hardness of the table he can discern himself "as being in a certain state—in the state, namely, of being aware of this hard table" (p. 17). Dawes Hicks, while standing in the relation of knowing to that particular table, has

perhaps special characters which are not present in him when he knows a primrose. And certainly he would have special characters when knowing which he would not possess when not in that relation at all, just as things which never enter into that relation lack qualities necessary for it. And the discovery of such characters would be part of the explanation of the relation of knowing, i.e. of the whole complex situation *A knowing B*. But the discovery of such characters is impossible while knowing is taken as the essence of mind, and not as a relation.

Dawes Hicks takes a continuum of such "highly specific acts of awareness" to be what we mean by mind. Their "specific" nature is actually given by the inclusion of the object known, or of its appearances in terms of Dawes Hicks's distinction. In this way he no doubt imagines that he keeps "the riches" of mind and its "hoarded wealth of mental suggestion", which, he complained, Alexander's "theory of consciousness as possessing only the single quality of consciousness" could not explain (p. 33-4). Although the theory by equating mind with its knowing, implies that it contains what is known, he attempts to make a complete distinction between material things and mental lives; they are, he says, "fundamentally disparate entities". Thus—"Were the awareness of shape not shapeless, the awareness of motion not motionless, and the awareness of colour not colourless, apprehension of the shape, motion and colour of things *would* be precarious indeed" (p. 41). Here again we may refer to other types of relation. It is by conceiving of "awareness" as something which is separable from the terms of the relation that it is conceived of in these ways, and by the same way of thinking we would say that "on-ness" and "nearness" were motionless, shapeless and colourless, although the terms of the relation had the features of shape, motion and colour. But if mind be distinguished from the complex situations of knowing, of which it sometimes forms a part, there is no reason why it should not be in motion, possess shape and colour, and yet be able to discover these same

properties in other objects. By his theory of awareness, Dawes Hicks's theory of knowledge, in spite of the amount of valuable material in it as regards the status of objects known, contains inconsistencies on that very point, and sets up a dualism between minds and their objects, such that he could not show how there could be any intercourse between them.

Dawes Hicks remarks in the Preface that he was for a long time perplexed with the phenomena of imagination. While feeling that the process of imagining was of one piece with both that of perceiving and that of conceptual thinking, nevertheless "so-called 'images' did seem to present a character which it was difficult to reconcile with the results of the analysis one was offering of the facts of perception" (p. vi). He confines himself to "objective" images which alone he takes as strict images. Thus he is not concerned with cases of memory or imagination where we do not seem to be apprehending a present object occupying some specific place. In spite of the element at least of illusion in these cases, Dawes Hicks describes the objects of imaging as "images", and argues that in an image there is a nucleus of vaguely apprehended fact round which gather features due to revived awareness of other objects. Imagination in fact is not to be absolutely distinguished from perception. As Dawes Hicks says, it is allowable for the sake of simplicity to treat perception in the first consideration of it, "as though it took place on each occasion de novo" (p. 95). But actually in perception there is a great deal which is not immediately discerned, but which in his phraseology "is discerned through the aid of the revival of previous awareness of similar objects" (p. 96). The recognition that what he calls "retention" or "revival" appears in perception is of importance (though Dawes Hicks's theory of the nature of mind gives him too easy a way out of the difficulties of explaining this fact), and it is clear that if this phenomenon can explain illusion in "imaging", it can also explain error in perception, though Dawes Hicks does not take up that question.

In line with his theory of perception, then, Dawes Hicks argues: " . . . in imagination, where objective imagery is

present, there is, as in perception, a real object upon which the act of discriminating is directed, and this accounts for the objective character which the content apprehended seems to possess, although the number of the features of this object actually discriminated is considerably less than in perception, and the portion of the apprehended content traceable to revived awareness considerably greater, and more arbitrary and haphazard" (pp. 99, 100). He has to show then that there is always some definite object or existent which is seen in this way, heard in this way etc., some of whose features are correctly apprehended. To show this he begins with the example of a child gazing at clouds and seeing them in the shapes of horses, chariots etc. Then he passes to images of taste, olfactory images, tactile images and kinæsthetic imagery. In all these cases he thinks it would be granted that the images always occur in conjunction with sense-perception, and so tests his theory by reference to auditory and visual images. In these cases, he suggests, the difficulty of distinguishing what are strictly images from what are not is what prevents the recognition that the former embody a nucleus of perceived fact. A strict case of imaging would presumably be the hearing a noise as coming from without, whereas when a tune, as we say, runs through our head, that is generally a case of "internal hearing". But it might be argued that the latter is a more puzzling phenomenon than the first. Although the tune does not seem to come from some "outside" source, it does seem to possess a certain objectivity in the sense of independence; it goes on regardless of "our" annoyance at it. It is not clear how saying that in such cases, as distinct from "objective" imagery, people "are capable of introspection in an eminent degree—in other words, of reflecting upon previous mental states of theirs" (p. 105) explains anything.

But the main question is whether Dawes Hicks's examples bear out his theory that in imaging we add features to those correctly though vaguely apprehended by us which belong to an object. What they do suggest is that there is perception

of some object, but hardly that the person connects with this further imagined features. The fact that it is when following the lines of music that a musician hears the notes (assuming that this is "objective", not simply "internal" hearing) hardly bears out the theory. To the musician the notes would not appear to be characteristic of the printed score. And similarly with Dawes Hicks's discussion of objective dream-imagery, which, he considers, especially confirms his theory. Granted that these images "originate in consequence of actual perception on the part of the individual", and that "the content, of which the individual is (in and through the mental act occasioned by the stimulus) vaguely and confusedly aware, suggests, or starts the suggestion of, those features that play the predominant part in the dream" (p. 110), nevertheless these images appear as quite unconnected with such stimuli. The sense-material actually given cannot be described as "the pivot around which the suggested 'imagery' is grouped and hence interpreted in objective fashion" (p. 115). The fact that attention to the imaginary object often causes it to disappear, and that in numerous instances the disappearance of the image results in "the real object exhibiting itself, and occupying the place previously occupied by the 'image'", certainly gives verification to Dawes Hicks's theory, but in many cases he would be faced with the same position as in the mirror-image example, namely the appearance of qualities in a place, without these appearing to inhere in any actual though vaguely discriminated object.

Nevertheless this part of his discussion is perhaps the most interesting in the book, whereas his suggested explanation of revival or retention is of little value. Since the content apprehended in a case of apprehension cannot persist after the act by means of which it has its being, it must be the content of the mental act that can be revived or reproduced. These contents go to constitute the very being of mind, and it has the power of retaining them in some form and reviving them. They cannot be brought back as objects, since they never were objects, but "we can say that it pertains to the

very being of a mind that it has the facility, in and through subsequent states, of 'reproducing' or 'recalling' the awareness which was the content of a previous state and of utilising this retained awareness in the life of the present and of the future" (p. 95). It would be better for Dawes Hicks to confess ignorance than to turn something he cannot explain into part of "the very being of a mind". These contents are subjective factors and his final question is how they affect the content apprehended so as "to give rise to constituents in it that seem to the imagining subject to be unmistakably objective?" (p. 116). While admitting the difficulty of answering this, he suggests that the phenomenon of Einfühlung "reading into impersonal and inorganic objects conscious feelings, emotions and desires which are really in ourselves" (p. 117), may throw some light upon it. The explanation of this is itself, he argues, yet to be found; all such terms as "externalisation", "transference", "projection", "projicience", he rejects as merely metaphorical, but suggests that whatever is the explanation of this will help in the explanation of the other problem, since in both cases it is contents or partial contents of mental acts which influence the content apprehended. But the "awareness of blue" etc. are not mental contents, as argued before; they are not on a level with peacefulness, anger, and seriousness. In fact, by his own theory, they cannot be of the same order, because while the anger which is supposed to be found in the object is actually found in the mind, in the "awareness of blue" there is no actual blueness residing in the mind, and so the explanation of the influence on the apprehended content would have to be radically different in the two cases.

The fifth essay, *Conceptual Thought and Real Existence*, hitherto unpublished, formed originally one of the Upton Lectures in Philosophy delivered at Manchester College, Oxford, 1933-4. It supplies more of the details of the logical position on which the earlier essays rest, and as in the case of imagination, Dawes Hicks's contention is that there is no absolute severance between perception and conception, but

"that thinking or judging is involved in cognitive activity throughout, that even a rudimentary act of cognition is already in essence an act of judging" (p. 122). But he considers especially the "extremely complex reflective acts, which are distinguishable from acts of perceiving and imagining . . ." (p. 122), that is, he is concerned with the definite search for resemblances, with scientific work. While insisting that "A connexion thought about is taken by the conscious subject to be an objective connexion—that is to say, a connexion in the nature of things, whatsoever those things may be" (p. 131), he finds it necessary to set up "concepts" as entities distinct from the universals to which they refer. "A concept is a way in which a universal is conceived, a mode in which it is grasped by thought, or, in other words, a content apprehended (corresponding to the so-called 'presentation' or 'appearance' in sense-perception)." And like an appearance it has a mysterious being, although Dawes Hicks, following Bradley, holds that "it does not happen" (p. 135). The general position on which both the theory of appearances and that of concepts rests is presented on pp. 138-141. It is Meinong's theory expressed by Dawes Hicks: "The whole of what exists, including what has existed and will exist, is, after all, infinitesimal when compared with the whole of the entities that are or may be known . . . we need to recognise that the term 'being' or 'reality' is a vastly wider term than the term 'existence'." Relations, as might be expected, are classed as such "subsistent" entities, also numbers, truths, and moral and æsthetic values. Many of these "subsistent" entities "are of tremendous moment for what exists, yet in and for themselves these entities can, in no sense, be said to exist". In fact for existents, qualities which are said to "subsist", are of such moment that the particular concrete entity, the "that" in Bradleian terms, although it is "something which *has* qualities or characteristics and is not a mere sum of characteristics or qualities . . .", cannot be perceived by us in abstraction from the qualities, the "what". We are said to perceive only the qualities of the thing, "yet this is, in itself,

no reason, of course, for doubting its presence". It seems curious that we should have such a "prejudice in favour of the actual", such a refusal to give full weight to the non-existent, when apparently that is all we ever perceive. And after the objection to Broad's theory that there is no proof of the existence of physical objects, the following comes strangely: "In the case, for instance, of a material thing, that which *has* the qualities would be the complex of atoms and molecules, the activity of which occasions the act of perceiving by which we become aware of its qualities but which is not itself revealed." And there is surely a deviation here from the position set out in "The Basis of Critical Realism" which placed scientific discoveries simply under the "more" which was not discriminated by unassisted perception.

The five essays considered fall together naturally as giving Dawes Hicks's view of cognition. In *The Dynamic Aspect of Nature* (1925) and *Professor Eddington's Philosophy of Nature* (1929) he deals with philosophical issues raised by modern science. *Is the Mind a Compound Substance?* (1926) is a criticism of Broad's theory that the mind is a compound of a bodily and a psychic factor, which when combined form a whole exhibiting mental properties. In these three essays he develops the dualism between minds and nature which appeared in the earlier essays. *The Refutation of Subjectivism* (1934) is a detailed consideration of Stace's "The Theory of Knowledge and Existence", and is included for the sake of the argument in the concluding paragraphs, where Dawes Hicks considers the general subjectivist position.

Dawes Hicks praises the detailed character of Meinong's work and such particularisation is evident in his own work, to the detriment of a consideration of logical questions which are nevertheless implied by those dealt with. Not that it is being contended that a philosopher should confine himself to logical questions, or to the theory of being. But it is contended that many of the distinctions set up in the course of epistemological enquiries, for example, are such as to make a

theory of being impossible, and that Dawes Hicks in particular sets up classes of entities which are entirely disparate, so that he could not explain their common nature as entities. Also that until the logical theory of relations in general is taken up, the explanation of knowledge in particular is unlikely to advance beyond the chaotic state in which it is at present.

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EDUCATING FOR DEMOCRACY. Edited by J. I. Cohen and R. M. W. Travers. London. Macmillan, 1939. Pp. xxx + 458. English price: 10s. 6d. net.

IN a work entitled *Human Affairs,* published three years ago, the present editors together with Mr. R. B. Cattell brought together some fifteen writers on the subject of the relation of science to society, the contributions that the various sciences might make to the solution of social 'problems' and to the formation of a social policy. *Educating for Democracy* applies the same method to the special field of education; it is intended to present, in the words of the editors, 'a cooperative effort to state the function of education in English democracy'. The truth is, however, that anyone who relied upon this as an accurate description of what the book contains, or who took the title seriously, would be somewhat misled. Of the twenty-three papers it includes, no more than three or four touch in more than incidental fashion on the question of what a democratic society might be, or what would be the distinctive features of a democratic education. Of course, much of the material presented by many of the writers suggests some conclusions as to whether any such thing as 'English democracy' really exists. And, again, it is not difficult to guess in most cases what view of democracy the writer would, if pressed, defend. But what one would expect in a book so named and described—namely, discussions of the theory of democracy and of the philosophy of education that would naturally accompany such a theory—are almost entirely absent. It is much too easily assumed (as, indeed, is

common among writers in all sorts of fields) that everyone knows what precisely democracy is; or that if one does not, he can easily find out by glancing at one of the surviving democratic models. Because of this quite unwarranted assumption, not many of these papers throw any light at all upon the problem suggested by the title, though that is not to say that they are not interesting enough in other ways. The editors' remark (in justification of their 'not having prepared a book on Socialism and Fascism'), that 'scientific research, as a social effort, is radical by its very nature, and will do as much as anything else to bring about transformations in society, without alienating people by the use of political notions', though it would not be accepted by some of the contributors, prepares us for the view taken by some of the others of the plane upon which discussion of educational theory can proceed. It also reminds us of the muddle of politics and science that was an outstanding feature of their earlier production. And it is hardly an appropriate introduction to a work that uses a 'political notion' in its very title.

It would be nearer the mark to say that it consists for the most part of short discussions of *special* educational problems and institutions. The first paper by Sir Percy Nunn on *Education as a Biological Experiment* develops a general theory of the nature of education, and Professor Kandel on *Comparative Education* considers the influence upon an educational system of national and social institutions of other sorts. For the rest, there is a group of chapters dealing with schools and educational institutions of various kinds—the nursery school, the different sorts of post-primary school, child-guidance clinics, non-university colleges, organisations for adult education and vocational guidance, and so on. Another group concerns the claims of special subjects to a place in the school curriculum. This side of the book seems to me to be unsatisfactory. The selection of subjects actually discussed appears to have been determined by what happened to be the main interest, or hobby, of the men invited to contribute; the result being that many subjects which, on any

view, would be regarded as part of the equipment of an educated man are not mentioned at all, and it falls to no one to attempt to sketch the plan of the complete curriculum of a satisfactory education. Finally (to complete our description of the scope of the book), there are accounts of such miscellaneous topics as school architecture, medical treatment and inspection, the economics of education in Great Britain, research in education, the influence of population changes upon the structure of the educational system. I would say that all but two or three of the papers are interesting and useful treatments of their special subjects. But although there has been some consultation among the contributors, it cannot be said that any coherent or continuous account of education and its problems emerges from these pages, or that the book is anything more than a collection of essays on diverse topics. In what follows, I shall make some general remarks on one or two important questions that are raised by one or other of the writers, instead of attempting the impossible task of doing justice to even a few of the separate papers.

Many of the contributors exhibit some dissatisfaction with English education, the feeling being apparently that education in Great Britain is not as closely in touch as it might be with the main movements of contemporary society. This view, and the contention that it is an essential function of education to initiate the young into the dominant movements of their society, is most fully expressed by Nunn. He argues that 'among modern nations, education has tended to become a stiff, if not a closed, tradition insufficiently responsive to the great changes and movements in the society it should serve'. In this respect, education is more alive in the fascist countries, where the authorities look to education 'both to secure what they have achieved and to provide for its future development'. In the democracies, the immediate task is 'to bring our education into close and vital relations with the deeper and more significant movements in our national life'. According to Nunn (and here again he expresses a view

that appears to be shared by the majority of his collaborators), this loss of contact has been largely due to the dominance of the literary and linguistic tradition in education, and to the consequent failure to advance science to its proper place in the educational scheme. 'The ordinary activities of the nation', Nunn argues, 'flow increasingly along channels prepared by science, are increasingly sensitive to the ever-changing movements of scientific discovery and are producing a world where the prevailing atmosphere is increasingly the atmosphere of science'; but in the schools 'there has been no reorientation at once corresponding in direction and comparable in extent with the reorientation in the general stream of the national life'. In this connection, Nunn refers to the well-known views of Lancelot Hogben on scientific as against literary education, and quotes his dictum that in modern society 'the spark-plug is mightier than the pen'. 'This implies', so Nunn concludes, 'some shifting of the centre of gravity of school studies. It must not lie, as of old, in the middle of the literary and linguistic group, though we need not seek to correct an old error by committing a new one, and bury it within a group of formal sciences to be given the same sort of dominance as the classical languages once enjoyed. . . . What is needed is not a great extension of formal science teaching, but a revaluation which will invest scientific studies with a dignity and importance answering to their significance in the activities of the nation. This will certainly result in giving a scientific colouring to the curriculum as a whole . . . but it need not imply that technical scientific studies should swamp the time-table.'

It seems to me, however, that there is some confusion in fashionable views concerning the social and educational importance of modern science, and also that the contributors to this volume fail, in general, to do justice to the case that might be made out for regarding the 'centre of gravity' of a liberal or democratic education as still to be found in studies of a literary and historical kind. But taking first the matter of the relation of education to social life, the point is also

worth making that at least one of the functions of education is the preservation of cultural activities and traditions, and that, under certain social conditions, this function could be carried out only if the educator is prepared to place himself in opposition to, or to hold aloof from, certain of the social forces and movements in his society. And it could be argued further that an education is democratic to the extent to which it does promote the activities of scientific inquiry and literary and artistic creation and appreciation, since these are activities which, under modern conditions, may be expected to provide one main source of opposition to anti-democratic tendencies and which themselves demand a democratic way of life. These, however, are points of which Nunn himself takes account; he recognises that in a democracy the schools will 'give their pupils access to all the main roads to culture', and also that, unless educational institutions set themselves to resist the elements of vulgarity and barbarism in modern civilisation, they are failing to perform their proper work. But in this, Nunn is considerably more enlightened than most of the other writers, and what one more commonly finds presented as a democratic education is education for one or other of the sorts of lives that are followed in a modern community; a number of different varieties of education adapted to the psychological and social 'needs' and 'capacities' of individuals. In the present book, this sort of position is put most fully by Professor Olive Wheeler, writing on *The Road through Adolescence*; what she is concerned to maintain is that the democratic 'equality of opportunity' should not be confused with the 'identity of provision'. As to this, I should say that 'equality of opportunity' is not especially a democratic conception; since it is clearly compatible with the existence of all sorts of social inequalities, or with a hierarchically and bureaucratically organised society. It is an individualistic slogan; it means usually no more than that competition in the social market for positions of social eminence should be free or unrestricted: it is the expression of a social *laissez-faire*. But the equality one thinks of as

characteristic of a democratic society consists in the absence of monopolies or 'appropriations' of political and social power, no matter how the groups which enjoy these monopolies may have been recruited; it means the participation of all members of a community in those political, social and cultural movements which determine the main lines of social development. On this view, then, educating for democracy must involve some 'identity of provision'; it must provide admission to the fundamental cultural activities of society, unless, indeed, educationists intend to accept as a *fait accompli* the undemocratic tendency for modern 'democratic' societies to be dominated by an 'élite' of intellectuals and technicians, the expertness of whom cannot be popularly appraised. And however great the difficulties confronting this sort of view, and no one would want to deny their existence, it seems clear, at any rate, that Professor Wheeler has not sufficiently considered what political and social equality would involve. One always expects to be told that there are *some* who are unfitted for a liberal or cultural education; but Professor Wheeler considers that it is the 'less-gifted majority' that requires an education free from an 'academic bias'. It is impossible to see how, if this becomes established as democratic educational policy, the widespread social, cultural and political initiative that is a part of a democratic view, could ever be achieved.

Now, as for the question of the relative importance of literary and scientific education, I should argue that it is by means of an education that centres around literary and historical studies that pupils are best brought into contact with the forces that control social life. No one would deny the importance of science in contemporary culture, or the contention that an educated man would be one competent to understand the fundamental principles and methods of science. Nor would one want to dispute the assertion of Mr. H. L. Beales that a 'classical' (and also a literary) 'education is an illiberal education'—as such an education has been commonly understood in Great Britain. This is because such an education has been so often treated simply as providing a

badge of social rank, or a sufficiently difficult initiation into an exclusive social class. Because of that, the opposition between a literary and a scientific education has been able to develop; the study of science being left to those who wanted to 'get things moving', to understand, to predict and to control. What would do more than anything else, perhaps, to restore vitality to literary and historical teaching would be the strengthening of the relations between it and the scientific movement, the extension of the speculative, investigatory and power-seeking attitude to this field also. No doubt, much of the teaching of classics, literature and history now seems dead and dusty; and good students commonly feel that it is the scientists who occupy the advance posts of culture, and who have the best opportunity of contributing to cultural and social progress. This feeling will have some justification unless history is studied politically, as a means of illuminating and contributing to present socio-cultural issues and movements.

Is it the case, as the editors claim, that science 'is the great force in the continuous transformation of society both in material production and culture'? It is, of course, indubitable that scientific and technical discoveries have not only greatly affected methods of communication and production, but have, in consequence, been one of the factors influencing social structure and the system of social relations. But it is also true, as Mr. Hugh Gaitskell points out in his article on *Economics and Modern Education,* that the 'social consequences of science are produced in and through a particular institutional framework'. As he quite correctly suggests, we could not understand the social history of modern science without an independent study of the institutions and forms of social relations which have conditioned its applications to economic and social processes. It could scarcely be pretended that science, whatever its social importance may have been, has created the social and legal order which constitutes the modern state; or again, that any amount of knowledge of modern science and technology would put us in a position to criticise social policies or philosophies. Thus, a study of

natural science and technology would not put us in touch with the controlling forces of social development; and if it appears that scientists and technicians are the rulers of modern society, as many seem to think, this is only because they have hitherto been content to work within the social and political framework of capitalism. If, therefore, it is the intention of our educators to bring the educated into touch with the fundamental forces and movements underlying social life, to make clear social and political mechanisms, a predominantly scientific education would be quite beside the mark. On the contrary, an education for social initiative or responsibility must be an education in social science, which is to say that it must be an education in social and cultural history. And such a study would make possible an understanding and criticism of science itself as a social and cultural movement.

It would include also certain of the subjects, for the admission of which into the curriculum, and for the greater emphasis on which, educationists are now contending. Thus, while it may be true, as Gaitskell argues, that it is important that a modern education should include economic history, a study of social and economic structure, and some consideration of contemporary social and economic problems, it appears that these things would be already provided for in the general study of history; it would certainly be a narrow course that excluded them. And, again, as regards the contention that 'psychology should form part of the general science course in every school', in order 'to diffuse a widespread scientific attitude towards the problems of human behaviour', it appears to me again that the study of history will itself, if it is worth cultivating at all, diffuse a scientific attitude towards social behaviour. And, further, the study of literature provides a more concrete introduction to human behaviour than would be found in any introduction to psychology, suitable to be taught in schools. Moreover, it is not an advance in education that problems which are capable of being treated in connection with one another, or as parts of one main subject, should be separated and distributed amongst

a number of specialists. What the educator is most concerned to show (and what, in fact, constitutes one of his most difficult problems) is the relevance of various studies to the central issues of social life and to one another, while the multiplication of special subjects and specialist teachers, even if it is to some extent unavoidable, leads to confusion of the fundamental with the secondary, and to a loss of the sense of culture as a whole.

I would maintain, therefore, that it is in the study of history that one would find the point of integration that some of the writers are searching for; that if it is a question of providing the materials for a critical understanding of social and cultural progress, historical studies will be the central thing. The weakness of the conception of the predominantly scientific education seems to me to be brought out in Mr. Richard Palmer's paper on *Science in Modern Education*. He, too, is alive to the defects of a formal training in special sciences, but it may be questioned whether the solutions he suggests can be accepted. 'If the citizens of tomorrow are to choose wisely', he argues, 'they must be alive to the technical forces that shape our civilisation, to the dangers that face it and to the new powers for good that lie within our grasp. . . . Our scientific education must aim to produce a generation that will *habitually* think of the knowledges in terms of human welfare. The basic assumption which must constantly be made the starting point of topics in science teaching is that the knowledges must be used to increase the happiness of all men. . . . The knowledges should be presented primarily as a bag of tools for fashioning the material bases of the good life'; and he goes on to suggest that 'this method of teaching provides an integrating motive to the informative side of the whole curriculum'. I cannot see, however, how any aspiration so vague as the aspiration for 'welfare' could give a point of integration as a guiding line for an approach to the theory of scientific education. There is nothing to inform us in advance what might be the relevance of any scientific investigation for 'welfare', and, in any case, it could not be pretended that much of the study necessary for the achieve-

ment of a scientific culture is not extremely remote from any consideration of practical use. No doubt men commonly become interested in some special field of study by seeing its bearing upon some present social 'need', but, in the schools, where it is a question of coming to terms with the fundamental principles of the sciences, the slogan of 'human welfare' could not but be an irrelevant gesture, without any immediate relation to the work actually being done. And it is unlikely that if students are not interested in the scientific material itself, they can be made interested, or persuaded that what they are doing is important, by attempts to suggest that science might be used as an instrument for the satisfaction of someone else's needs. But, what is more important, it may be asserted that the approach from the side of 'social possibilities', and 'material welfare', is more likely to prevent the growth of an interest in science and in culture; in so far as such teaching was effective, it would tend to oppose itself to the development of purely scientific and cultural interests. If it is part of the purpose of the educator to exhibit the character of science as a social force, the insistence on science as an instrument for the satisfaction of needs, would rather obscure the role that science has played and might play in the whole field of social and cultural development.

In these remarks, it has been possible to touch, and that quite dogmatically, upon only a few of the issues raised in the book. It will not have been made clear that it presents a detailed account at least of contemporary developments of educational practice. What I have tried to suggest is that it does not contain any very enlightening or satisfactory discussion of the final questions of educational theory or philosophy. And this because the writers, as with most educationists, are not prepared to plunge into the jungles of social and political philosophy. A philosophy of education is possible only as a sequel to a social philosophy, and while there is little fruitful thinking about the fundamentals of social theory, it cannot be expected that any very far reaching contributions will be made to the theory of education.

P. H. PARTRIDGE.

JOURNALS RECEIVED.

MIND: A QUARTERLY REVIEW OF PSYCHOLOGY AND PHILOSOPHY. Macmillan & Co. Annual subscription: 16s.

Vol. XLIX. No. 195, July, 1940. On Probability: G. H. von Wright. The Ethical and Social Thought of Protagoras: Adolfo Levi. Knowledge, Reality and Objectivity (II): H. F. Hallett. The Nature of Entailment: Norman Malcolm.

THE JOURNAL OF PHILOSOPHY. Published fortnightly. Columbia University, New York. Subscription: $4 a year.

Vol. XXXVII. No. 10, May 9, 1940. The Essence of Peirce's System: Paul Weiss. Comments and Criticisms—The Accidents of Peirce's System: Justus Buchler; Concerning the Emphasis on Methods: J. A. Lynch. No. 11, May 23. What is Empirical?: J. Loewenberg. Discussion of Professor Loewenberg's Paper: Percy Hughes. Objects Perceived and Objects Known: Albert G. Ramsperger. Criticism and Fixed Species: Robert McRae. No. 12, June 6. Concerning the Status of so-called "Pseudo-Object" Sentences: C. J. Ducasse. Modern Art and Social Responsibility: William J. Norton, Jr. No. 13, June 20. The Consequences for Metaphysics of Quantum-Mechanics: C. H. Kaiser. The Meeting of Extremes in Recent Esthetics: Robert C. Baldwin. No. 14, July 4. An Experimental Critique of Rationalistic Ethics: Robert E. Fitch. Two Meanings of Liberty: Philip Blair Rice. Individualistic and Collectivistic Liberty: Manley H. Thompson, Jr. No. 15, July 18. Operationism, Construction, and Inference: Charles E. Bures. Selfishness and Unselfishness: George Morgan, Jr. No. 16, August 1. Conceptual Relativity: Laurence J. Lafleur. The Spirit of the New Positivism: Virgil C. Aldrich.

THE PHILOSOPHICAL REVIEW. Published every two months. Cornell University, Ithaca, N.Y. Annual subscription: $5.

Vol XLIX. No. 3, May, 1940. The Cartesianism of Phenomenology: James Street Fulton. Professor Alexander's Proofs of the Spatio-Temporal Nature of Mind: J. V. Bateman. Royce's Conception of Experience and of the Self: Diana Monsman. No. 4, July. Diocles of Carystus—a New Pupil of Aristotle: Werner Jaeger. Cournot's Doctrine of Philosophical Probability: Chester Townsend Ruddick. Some Common Misinterpretations of the Kantian Ethics: H. H. Schroeder.

PHILOSOPHY. Journal of the British Institute of Philosophy. Published quarterly. Macmillan & Co. Annual subscription: 14s.

Vol. XV. No. 59, July, 1940. In Memoriam—John Henry Muirhead: A.D.L. The Ethics of Pacifism: A. D. Ritchie. Hegel's

Dialectic in Historical Philosophy: J. O. Wisdom. Archimedes and Eddington: G. Burniston Brown. "In the Beginning": B. A. Farrell.

REVUE PHILOSOPHIQUE. Published every two months. Alcan, Paris.

Vol. LXV. Nos. 1 and 2, January-February, 1940. A la Mémoire d'un grand philosophe, Edmund Husserl: L. Chestov. L'oeuvre d'Edmond Husserl: E. Levinas. "Cheminements et Carrefours", par Rachel Bespaloff: J. Wahl. La liberté de l'artiste: G. Picon.

THE JOURNAL OF SOCIAL PSYCHOLOGY. The Journal Press, Provincetown, Massachusetts. Annual subscription: $7.

Vol. XI. Second Half, May, 1940. Sentiments and Attitudes: Peter A. Bertocci. A test for personal goal-values: Frederic Wickert. The interrelationships of some general and specific preferences: Frederic Wickert. Community of values as a factor in friendships of college and adult women: Helen M. Richardson. Dominance-quality and social behavior in infra-human primates: A. H. Maslow. Parental attitudes of farm, town, and city parents in relation to certain personality adjustments in their children: Leland H. Stott. Changes in the opinions of female students after one year at a university: Stephen M. Corey. The study of "populistic painters" as an approach to the psychology of art: Ann Anastasi and John P. Foley, Jr. Stated behavior vs. stated opinions as indicators of social-political-economic attitudes: C. Robert Pace. The construction of a scale for measuring attitude toward militarism-pacifism: Murray Gristle. Propaganda and opinions on foreign policy: Selden C. Menefee and Audrey G. Granneberg.

ANNALE DELLA R. SCUOLA NORMALE SUPERIORE DI PISA. Quarterly.

Vol. IX. Nos. 1 and 2, 1940. Marganorre: Enrico Carrara. Lessico giuridico latino e tradizione mediterranea: Giovanni Nencioni. Della "crestomazia italiana" del Leopardi e di altre antologie: Emilio Santini. Pedagogismo assoluto: Vl. Arangio-Ruiz.

NOTES AND NEWS.

CONGRESS OF THE ASSOCIATION.

The annual congress of the Australasian Association of Psychology and Philosophy was held in Sydney University on the 14th, 15th, 16th and 17th August. The meetings were well attended, and discussion was lively and informative. The Association was fortunate in having a paper

(published in this issue) from Emeritus Professor G. F. Stout, who is now resident in Sydney. The programme was as follows:

First Session—Wednesday, 14th August.

10.30 a.m.—"History and Consciousness." Professor John Anderson (Sydney).

Chairman: Professor A. P. Elkin.

Second Session—Wednesday, 14th August.

2.15 p.m.—"The Concept of Perseveration." Mr. K. F. Walker (Sydney).

Chairman: Dr. D. Howie.

Third Session—Wednesday, 14th August.

7.45 p.m.—"Space and Time, Continuity and Causality in Modern Science." Dr. I. Rosenthal-Schneider (Sydney).

Chairman: Professor John Anderson.

Fourth Session—Thursday, 15th August.

11 a.m.—"Mathematics and the World." Mr. D. A. T. Gasking (Brisbane).

Chairman: Mr. G. A. Paul.

Fifth Session—Thursday, 15th August.

2.15 p.m.—"Things, Predicates and Relations". Emeritus Professor G. F. Stout (St. Andrews).

Chairman: Professor A. Boyce Gibson.

Sixth Session—Thursday, 15th August.

7.45 p.m.—"The Goodness of Producing and the Good Produced." Professor A. Boyce Gibson (Melbourne).

Chairman: Professor A. K. Stout.

Seventh Session—Friday, 16th August.

10.30 a.m.—"Moore's *Principia Ethica*." Mr. G. A. Paul (Melbourne).

Chairman: Professor John Anderson.

Eighth Session—Friday, 16th August.

2.15 p.m.—"The Problem of Analysis in Psychology." Dr. D. Howie (New England).

Chairman: Mr. A. G. Hammer.

Ninth Session—Friday, 16th August.

7.30 p.m.—Annual Meeting. Presentation of Annual Report and Balance Sheet.

8 p.m.—Presidential Address: "Freewill." Professor A. K. Stout (Sydney).

Chairman: Mr. P. H. Partridge.

Tenth Session—Saturday, 17th August.

10.30 a.m.—"Contradiction." Mr. D. Taylor (Melbourne).

Chairman: Mr. D. A. T. Gasking.

Eleventh Session—Saturday, 17th August.

2.15 p.m.—"Philosophy and Sociology." Mr. P. H. Partridge (Sydney).

Chairman: Professor A. Boyce Gibson.